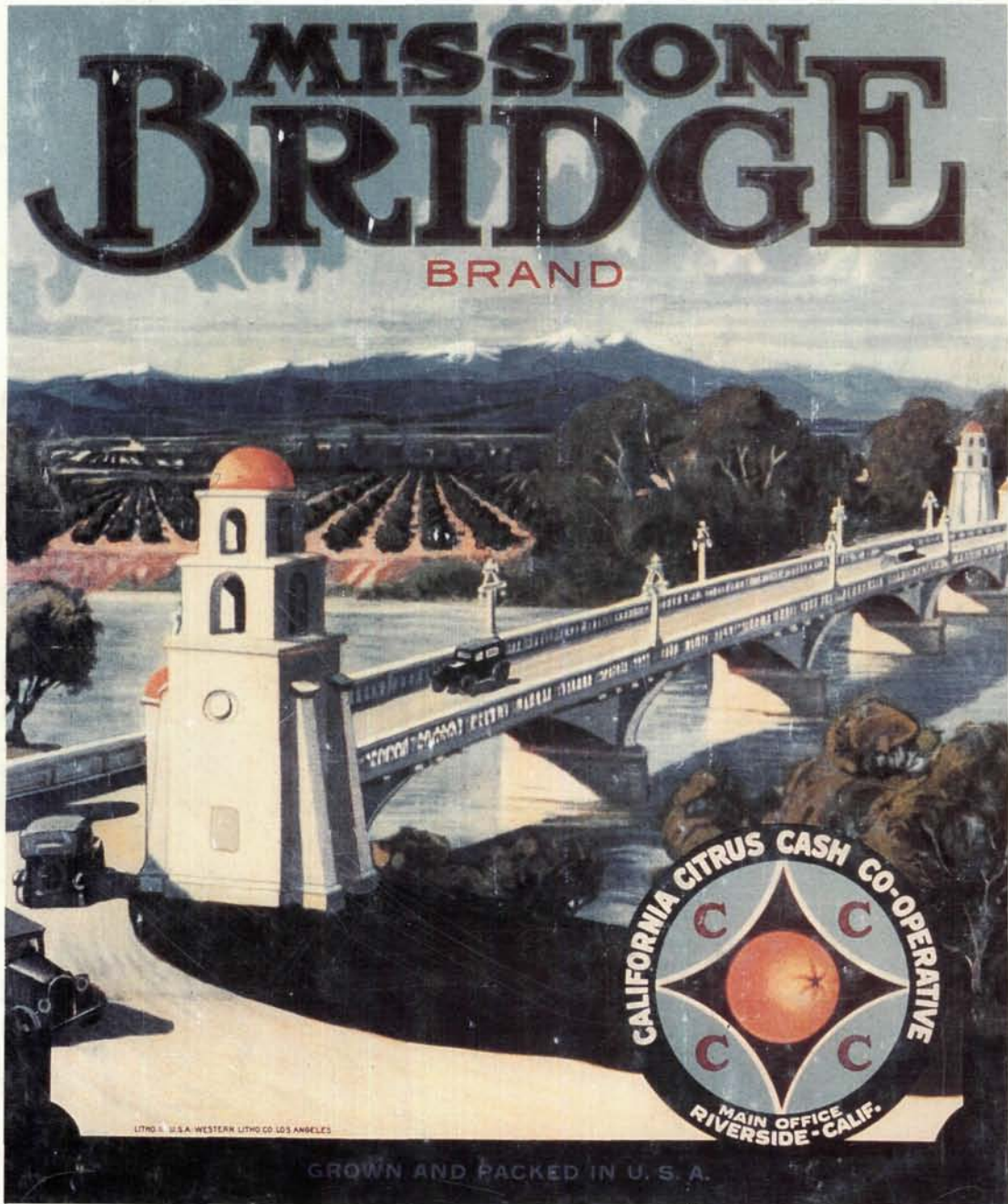


VOLUME 95 • NUMBER 5 • DECEMBER 1990

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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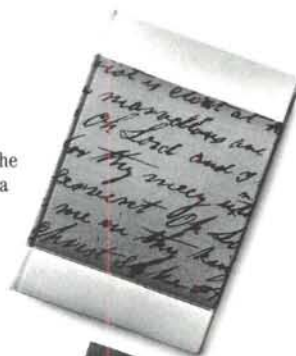
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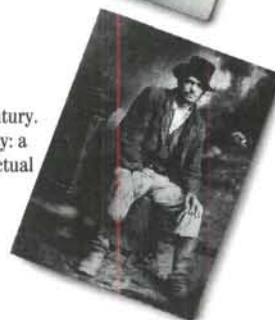
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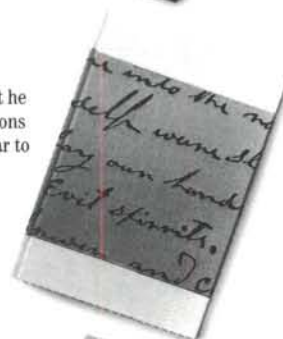
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Cover illustration: The paper label of an orange crate produced for the California Citrus Cash Co-operative, Riverside, California, by the Western Litho Company, Los Angeles, circa late 1920s.

See the article on Riverside in this issue by Ronald Tobey, Charles Wetherell, and Jay Brigham, "Moving Out and Settling In: Residential Mobility, Home Owning, and the Public Enframing of Citizenship, 1921–1950," pp. 1395–1422.

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In This Issue

All historians understand in a general way the importance of geography; it provides a stage for the events they describe. Only a few historians consciously use geographers' methods and insights in their historical research. This is changing. A new interest in combining historical and geographic analysis is evident, and this issue features three such studies.

Carl Abbott leads off with an analysis of regional cultural and economic influences in the history of Washington, D.C. He finds that while the capital has lost some of its original southern character and become more closely tied to the northeast in the past hundred years, it has also reinforced many southern connections and become southern in new ways. By demonstrating the importance of social and cultural influences, Abbott's study shows the inadequacy of unidirectional models of regional economic change. He suggests that Washington's experience offers a model for analyzing other border cities.

Until 1945, residential mobility in the United States was high. Historians have usually assumed that the postwar slowdown in mobility was reached gradually during the preceding decades. But, by analyzing a novel source—electrical utility billing records—**Ronald Tobey, Charles Wetherell, and Jay Brigham** are able to show that the postwar drop was sudden and dramatic, a nearly 50 percent reduction. They argue that government-insured long-term mortgages caused the change and that President Roosevelt designed his New Deal housing policies to achieve this swift transformation of a central feature of the American historical experience.

Peter Sahllins rescues the powerful idea of France's natural frontiers from the dustbin of history to which it was consigned by Gaston Zeller and other French scholars of this century. Zeller discounted the importance of the doctrine of natural frontiers to discredit German and French militarism in the pre-World War II era. Sahllins reexamines the issue, however, and, in considering the range of meanings and representations of natural frontiers deployed by French leaders and thinkers from Cardinal Richelieu's time to the revolution and beyond, contends that this notion (though its content and focus shifted) played an important role in French state building.

Two review articles follow. **Isser Woloch** takes on *A Critical Dictionary of the French Revolution*, edited by François Furet and Mona Ozouf, the most serious and insistent reassessment of that great event to emerge from the bicentennial literature. Furet and his collaborators consider the revolutionary ideology to be deeply flawed, and with it the revolution's institutions and practices. Their view emphasizes fanaticism over pragmatism and utopian visions over reformist policies. Woloch acknowledges their forceful arguments but registers strong reservations about many of them in an attempt to restore the kind of balanced appraisal of the revolution that he finds lacking in the book itself.

Judith Leavitt reviews fourteen new studies in the history of medicine. She notes that scholars have abandoned the former teleological model that focused on intellectual change and have adopted a contextual view of health care practices emphasizing the social situation of the patient and the healer. Several of the books she reviews give special attention to issues of race, class, and gender.

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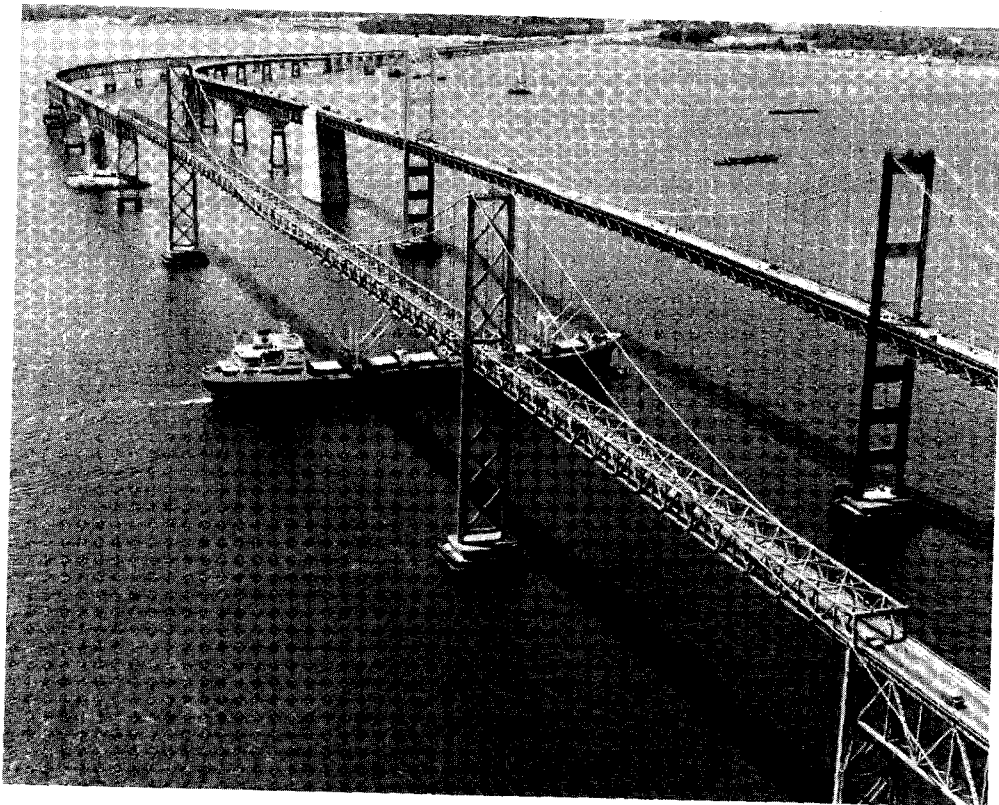
Peter Sahlin is an assistant professor of history at the University of California, Berkeley. He received his doctorate from Princeton University and is the

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Opened in 1952 and augmented with a second span in 1973, the Chesapeake Bay Bridge offered residents of metropolitan Washington easy access to the Eastern Shore of Maryland for recreation. By 1980, several Eastern Shore counties were part of Washington's vacation home hinterland. Photo courtesy of the Maryland Transportation Authority.

Dimensions of Regional Change in Washington, D.C.

CARL ABBOTT

In *The Last of the Southern Girls*, NOVELIST WILLIE MORRIS created a Washington dinner party that seated Arkansas-bred Carol Templeton Hollywell next to a famous midwestern writer.

"Have you ever written about Washington?" [she asks amid the clatter of china and silver.]

"No," [is the writer's answer.] "Everybody's too native to somewhere else."

"Northerners consider it Southern and Southerners think it Northern."

"That's why it's here."¹

Morris's version of Washington small talk is good journalism but weak history. The common uncertainty about the city's regional identity in both local conversation and scholarly description is the result of historical development, not original intent.

When Thomas Jefferson and Alexander Hamilton struck their dinner-table bargain in 1790, they knew exactly what they were trading—federal assumption of largely northern state debts for a southern capital. The tidewater reaches of the Potomac River lay within the core of the colonial Chesapeake culture area. The new town took its early tone from the southern ports of Alexandria and Georgetown. The imprint of the colonial South was evident among Washington's elite in a taste for race tracks and gambling, the prestige of lawyers and placeholders, attention to social caste, and other values of plantation society.² The federal government also drew the new city southward. Southerners served as speaker of the House of Representatives for forty-one of the first fifty years that Washington was the seat of government, as president for forty-two, and as chief justice for fifty. To the tidy New England eye of young Henry Adams, the capital as late as the administration of Virginia-born Zachary Taylor was little more than a slovenly southern village.³

Since 1860, in contrast, Washington has grown from a regionally rooted community of 61,000 into an ambiguously situated metropolis of 3.5 million. Its growing role in national administration and international relations and the accompanying economic and demographic changes have created a city that is doubtful about its own character. Observers have commented time and again that

Research for this essay was assisted by the Center for Washington Area Studies, George Washington University. Howard Gillette, Jr., Dennis Gale, Brett Williams, and Robert Liebman offered valuable comments during the course of the research and writing.

¹ Willie Morris, *The Last of the Southern Girls* (New York, 1973), 62.

² Frederick Gutheim, *The Potomac* (New York, 1968), 248; Constance McLaughlin Green, *Washington: Village and Capital* (Princeton, N.J., 1962), 109, 231; Wilbur Zelinsky, "Where the South Begins: The Northern Limit of the Cis-Appalachian South in Terms of Settlement Landscape," *Social Forces*, 30 (December 1951): 172–78.

³ Henry Adams, *The Education of Henry Adams* (New York, 1931), 44. Also see George A. Townsend, *New Washington: or, The Renovated Capital City* (Washington, D.C., 1874), 2, 7.

the city "used to be southern until . . ." The cliché centers attention on the influx of outsiders and the influence of outside values that have supposedly changed the character of the city, but its timing is slippery. The turning point is sometimes put as far back as the Civil War or the Gilded Age. Others date the big change to World War I, or the New Deal, or World War II, or racial integration during the 1950s, or the New Frontier, or home rule in the 1970s.⁴ The residual southernness of metropolitan Washington remained a commonly invoked explanation of local behavior and politics through the 1980s.⁵ We are left, in popular imagery, with a city of uncertain regional allegiance that is caught in protracted transition from southern town to something else, whether northern city, modern metropolis, or world capital. In a phrase attributed to John F. Kennedy, it retains the ambiguity of a city with "southern efficiency and northern charm."⁶

In itself, the regional identity of Washington is a minor historical puzzle posed by the volume of commentary on the national capital. Washington has been a special case among American cities in terms of its economic base, local government, and land-use planning. Nevertheless, the exploration of regional relationships leads to conclusions that are not obvious from its role as the federal city. Washington as an everyday community has remained embedded in its regional environment. Despite frequent comments about its isolation from the American mainstream, its residents have maintained many old regional relationships while accommodating and constructing new claims and connections.

This essay poses two substantive questions: When have major changes occurred in the city's regional orientation? What has been the character of such changes? The answers provide the opportunity to explore broader dynamics of regional change in the United States, for Washington has developed within the superimposed border zones of two distinct regional systems. As already noted, it lies along the historic boundary between the cultural North and cultural South. It has likewise grown within the shifting margin between the nation's economic core and periphery.

The popular understanding of Washington's gradually fading southernness is based on perceptions of change and stability in observable expressions of community values. Some of these expressions involve the realm of individual behavior revolving around family obligations and manners. Mississippian John Stennis, for example, is not the only observer who has pointed to the slow erosion in contemporary Watergate Washington of its traditionally "southern attitudes in the

⁴ Citations of different turning points include, for the Civil War and Reconstruction, Atlee Shidler, quoted in Barbara Palmer, "Myths of Washington," *Washingtonian*, 15 (January 1980): 90; for World War I, Fletcher Knebel, "Washington: America's Home Town," *Look*, 24 (April 26, 1960): 36-41; for the New Deal, Federal Writers Project, *Washington, D.C.: A Guide to the Nation's Capital* (New York, 1942), 3; and Ben Bagdikian, "The Five Different Washingtons," in Bill Adler, ed., *Washington: A Reader* (New York, 1967), 262-65; for World War II, David Brinkley, *Washington Goes to War* (New York, 1988); for the 1950s, Michael Frome, *Washington: A Modern Guide to the Nation's Capital* (Garden City, N.Y., 1960), 9; and Joseph Rauh, quoted in *Washingtonian*, 21 (Oct. 1985): 248; for the 1960s, Haynes Johnson, "The Capital of Success," *Washington Post Magazine*, February 2, 1986; for the 1960s and 1970s, Sam Smith, *Captive Capital: Colonial Life in Modern Washington* (Bloomington, Ind., 1974), 17; for the 1970s, Julius Hobson, Jr., quoted in David Ruffin, "Washington, D.C.: A Thriving Center of Power and Culture," *Black Enterprise*, 17 (May 1987): 78. The publishers of the standard national and regional *Who's Who* series shifted the District of Columbia out of the South/Southwest volume in 1976-1977.

⁵ Lee May, "Socially, Color Line Is Still Drawn in Nation's Capital," *Los Angeles Times*, January 17, 1989; "Once Again Snowstorm Halts U.S. Government," *New York Times*, January 27, 1987; "Washington, DC: A Second Revolution," *The Economist*, 303 (April 16, 1988), special section 1-18.

⁶ John F. Kennedy quoted in Arthur M. Schlesinger, Jr., *A Thousand Days* (Boston, 1965), 673.

social realm—neighborliness, friendliness, conviviality.”⁷ Expressions also include the domain of civic action. The enforcement or mitigation of racial segregation, for example, is taken as obvious evidence of allegiance to sets of values historically associated with the South or North.

Most commentators have found the source of behavioral and attitudinal changes in the emergence and expansion of a domineering economic core region within the northeastern quadrant of the United States. The centralization of industrial production, distribution networks, and control functions after the Civil War integrated the South and West into a single economic system. Because of the spatial congruity of the economic core and the northern cultural region, such national incorporation has been taken to imply northernization. In theoretical terms, the process is seen as an example of the unidirectional effects of modernization in a centralized nation state.⁸ In specific terms, the inclusion of the South within national developmental networks has been used to explain and forecast changes in social patterns and political behavior, as in C. Vann Woodward’s famous discussion of the changes wrought by the “bulldozer revolution.”⁹ Because of its proximity to the industrial core, the upper South has seemed particularly subject to such absorption within an expansive Middle Atlantic region.¹⁰

The history of Washington can help to delineate the two regional dynamics. Its evolution can tell us something about the relative strengths of northern and southern values and also about the capacity of economic modernization to alter cultural affiliation. As communication centers and central places, border cities can reach into hinterlands and migration sheds on both sides of a regional boundary. Washington’s problematic location therefore acts as a lens to focus the effects of regional contact and interaction and makes it a particularly useful case for testing alternative conceptions of regional change. Specifically, the changing character and connections of Washington may suggest ways to understand change and continuity in the larger South from which the city took its original character. Washington’s experience also directs attention at other cities balanced in similar ways between South and Middle West or South and West. Washington, Cincinnati, Dallas, and other domestic border cities as a group may have roles and character significantly different from those of regionally centered, economic and cultural capitals such as Boston, Chicago, or Salt Lake City—a set of American Strasbourgs and Triestes, perhaps, to contrast with the American equivalents of Paris and Rome.

⁷ John Stennis quoted in Howard Means, “The Northernization of Washington,” *Washingtonian*, 13 (August 1978): 83.

⁸ For a theoretical statement of the unidirectional diffusion of culture and values along with political and economic control in a modernizing society, see Edward Shils, “Centre and Periphery,” in *The Logic of Personal Knowledge: Essays Presented to Karl Polanyi* (London, 1961), 117–30; and Neil J. Smelser and Seymour Martin Lipset, *Social Structure and Mobility in Economic Development* (Chicago, 1966), 23–29. For critiques, see Pierre Clavel, *Opposition Planning in Wales and Appalachia* (Philadelphia, 1983), 39–72; and Sidney Tarrow, *Between Center and Periphery: Grassroots Politicians in Italy and France* (New Haven, Conn., 1977), 15–46.

⁹ C. Vann Woodward, *The Burden of Southern History* (Baton Rouge, La., 1960), 6. Also see V. O. Key, Jr., *Southern Politics in State and Nation* (New York, 1949), 671–75.

¹⁰ John Fenton, *Politics in the Border States* (New Orleans, 1957); Ira Sharkansky, *Regionalism in American Politics* (Indianapolis, Ind., 1970).

FOR MORE THAN A CENTURY, every increase of eight civilian jobs in the federal government has included one position in the Washington area.¹¹ Through the employment multiplier, new federal salaries have supported the growth of residential employment in retailing, services, construction, finance, and local market manufacturing. Expanding federal bureaus have attracted a complementary set of private-sector "national capital functions" whose payrolls have also fed the local multiplier.¹² The first period of concentrated change began with the Civil War and continued into the 1880s, with regional consequences emerging over the next forty years. The second period spanned the Great Depression, World War II, and the onset of the cold war, again followed by a full generation of regional readjustment.

I start with the assumption that cultural and economic models of regional patterning are complementary rather than exclusive, since both types of pattern are manifested by shared behaviors that structure the perceptions and orientations of individuals. The essay focuses midway between individual Washingtonians and the metropolitan community as a whole. The explicit commentary left by a minority of residents and observers can be supplemented by an examination of patterns and structures of social, economic, and political behavior that have conditioned or impinged on the activities of everyday life. Such mediating structures have helped to form Washingtonians' predispositions, orientations, and habits of mind and thus influenced the ways in which they have understood and responded to the regional character of their city. I try to describe such structures by asking a series of questions about individual perceptions, expectations, and experiences. What regional terms have Washingtonians found in the names of local businesses? What sorts of neighbors have residents been most likely to encounter? Where have local entrepreneurs expected to find out-of-town customers? The answers help construct a history of regional orientation in which incorporation in national systems ironically has reinforced connectivity and identification with both the North and the South.

ONE OF THE BASIC CONDITIONS OF AMERICAN DEVELOPMENT has been the early establishment of a set of cultural regions arrayed from north to south along the Atlantic coast and their consequent westward expansion along roughly parallel corridors of settlement. The analysis of such cultural regions emphasizes long continuities in the spatial distribution of values, customs, and other cultural information. English, French, and Spanish settlers brought sets of values and patterns of behavior to the New World. They responded to particular resource endowments by adapting this cultural heritage to the limits set by the world economy of the seventeenth and eighteenth centuries. In the British continental colonies, the result was distinct cultural areas around cores in New England, the

¹¹ Washington has been the location for a remarkably consistent proportion of federal civilian jobs since the Civil War. Since 1871, the decennial figures have ranged from 10 to 14.3 percent, with a mean of 12.25 percent. Grouped in forty-year periods, the means were 5.7 percent for 1831–1861; 12.6 percent for 1871–1901; 12.5 percent for 1910–1940; and 11.1 for 1950–1980.

¹² Council for Economic and Industry Research, *Economic Base Survey for the General Development Plan, National Capital Region* (Washington, D.C., 1956); Hamer and Company, Associates, "Economic Development in the Washington Area," staff study for the Joint Committee on Washington Metropolitan Problems, Congress of the United States, 1958; Metropolitan Washington Council of Governments, *An Economic Profile of the Washington Region* (Washington, 1980).

Delaware Valley, the Chesapeake Tidewater, and South Carolina.¹³ Expansion of settlement during the eighteenth and nineteenth centuries carried New England ways into the basin of the Great Lakes, Mid-Atlantic society into the Ohio and central Mississippi valleys, and intensive plantation society across the Gulf states. The Chesapeake region contributed to the southwestward expansion of slave society and also interacted with Mid-Atlantic traditions to create an "upland South" or "southern Midlands" in Kentucky and Tennessee.¹⁴ Beyond the 96th meridian, the historic cultural regions have overlapped and mingled even more extensively with each other, with distinct Hispanic and Native American cultural areas, and with separately established Anglo-American settlement systems spreading from Utah and California.¹⁵

The elucidation of cultural regions has been the province of a wide range of disciplines that emphasize the determining role of shared values. Important definitions of cultural regions have been offered by specialists in American studies, anthropology, architectural history, cultural geography, linguistics, material and folk culture, political history, and sociology.¹⁶ Such writers emphasize the early definition or spread of cultural regions, with secondary attention to current expressions of the heritage of early settlement in contemporary regional patterns. Their methods tend to center on the examination of homogeneity in the spatial distribution of traits and behaviors that express common heritage or values. The most detailed and influential descriptions of modern cultural regions by Wilbur Zelinsky and Raymond Gastil follow what Gastil has called the "doctrine of first effective settlement." His work divides the United States into cultural regions defined by "variations in the cultures of the peoples that dominated the first settlement and . . . secondarily by variations in the cultures of peoples that dominated later settlements."¹⁷

¹³ Donald W. Meinig, *The Shaping of America: A Geographical Perspective on 500 Years of History*, Vol. 1: *Atlantic America, 1492-1800* (New Haven, Conn., 1986); Robert D. Mitchell, "The Formation of Early American Cultural Regions," in James Gibson, ed., *European Settlement and the Development of North America* (Toronto, 1978), 75.

¹⁴ The trans-Appalachian expansion of cultural regions is discussed in Mitchell, "Early American Cultural Regions"; Henry Glassie, *Patterns in the Material Folk Culture of the Eastern United States* (Philadelphia, 1968); Terry G. Jordan and Lester Rowntree, *The Human Mosaic*, 3d edn. (New York, 1986), 10. Examples of detailed analysis include Richard L. Power, *Planting Corn Belt Culture: The Impress of the Upland Southerner and Yankee in the Old Northwest* (Indianapolis, Ind., 1953); Terry G. Jordan, "The Imprint of the Upper South on Mid-Nineteenth Century Texas," *Annals of the Association of American Geographers*, 57 (1967): 667-90; and Donald Meinig, *Imperial Texas* (Austin, Tex., 1969).

¹⁵ The growth and interaction of cultural regions in the American West is treated by Donald Meinig in "The Mormon Culture Region," *Annals of the Association of American Geographers*, 55 (1965): 191-220; *Southwest: Three Peoples of Geographical Change* (New York, 1971); and "American Wests: Preface to a Geographical Introduction," *Annals of the Association of American Geographers*, 62 (1972): 159-84.

¹⁶ Glassie, *Material Folk Culture*; Conrad Arensberg, "American Communities," *American Anthropologist*, 57 (December 1955): 1143-62; Hans Kurath and Raven I. McDavid, Jr., *The Pronunciation of English in the Atlantic States* (Ann Arbor, Mich., 1962); Fred Kniffen, "Folk Housing: Key to Diffusion," *Annals of the Association of American Geographers*, 55 (December 1965): 549-77; Peirce F. Lewis, "Common Houses, Cultural Spoor," *Landscape*, 19 (January 1975): 1-22; Terry G. Jordan, *Texas Log Buildings* (Austin, Tex., 1978); Wilbur Zelinsky, "An Approach to the Religious Geography of the United States: Patterns of Church Membership in 1952," *Annals of the Association of American Geographers*, 51 (June 1961): 139-93; James Shortridge, "Patterns of Religion in the United States," *Geographical Review*, 66 (October 1976): 420-34; Edward T. Price, "The Central Courthouse Square in the American County Seat," *Geographical Review*, 58 (January 1968): 29-60; Daniel Elazar, *American Federalism: The View from the States* (New York, 1972).

¹⁷ Raymond Gastil, *Cultural Regions of the United States* (Seattle, Wash., 1975), 26-33, 180, 190, 196; Wilbur Zelinsky, *The Cultural Geography of the United States* (Englewood Cliffs, N.J., 1973), 77-82; also see Bruce Bigelow, "Roots and Regions: A Summary Definition of the Cultural Geography of America," *Journal of Geography*, 79 (November 1980): 218-29.

The spread of popular culture studies in the last two decades has added twentieth-century examples to the extensive literature on folk culture in earlier centuries. Much of this work is summarized in the 387 maps published in *This Remarkable Continent*, a compilation organized by the Society for a North American Cultural Survey, an informal consortium of geographers, anthropologists, folklorists, and related specialists.¹⁸ Many of the newer expressions fit within the framework of traditional cultural regions. Country and western music has evolved and flourished within the upland South. Political support for environmental protection and participation in high school soccer have been concentrated within the northeastern core and its westward extension along the New England settlement corridor. Other cultural choices, such as membership in Christian Science churches, popularity of Americanized ethnic cuisines, and participation in high school football appear to reflect new regional dynamics that ignore traditional cultural boundaries.¹⁹

Within nearly all of the older studies and many of the new, the South appears as a historically stable region rooted in its resource base and ethnic composition. An important trend in recent southern scholarship argues long continuities in regional traits. Titles like *The Enduring South*, *Why the South Will Survive*, *The Lasting South*, and *Place over Time: The Continuity of Southern Distinctiveness* state the theme. Civil war, industrialization, urban growth, and national mass communication may have added new elements to the experience of the South, but its boundaries and essential character as defined by religion, language, rurality, and accommodation to racial variety have remained remarkably stable.²⁰

A number of writers on Washington have focused on these traditional "southern qualities" as expressed in the city's everyday life and their survival or attenuation in the face of modernization. Frederick Gutheim's volume *The Potomac* (1949), in the "Rivers of America" series, reveals his deep interest in cultural change. Gutheim described Washington's gradual northernizing during the nineteenth century and concluded that, by 1900, "those who came from the North continued to speak of its previously southern quality, but few Southerners did; to them it was the North." Constance Green's detailed history of Washington similarly emphasizes daily behavior and customs, public values, and race relations. "The relatively slow pace of life, deemed to be a southern quality born of the climate," she argued in one

¹⁸ John F. Rooney, Jr., Wilbur Zelinsky, and Dean R. Louder, eds., *This Remarkable Continent: An Atlas of United States and Canadian Society and Cultures* (College Station, Tex., 1982).

¹⁹ Ary J. Lamme III, "From Boston in One Hundred Years: Christian Science 1970," *Professional Geographer*, 23 (1971): 330; Wilbur Zelinsky, "The Roving Palate: North America's Ethnic Restaurant Cuisine," *Geoforum*, 16 (1985): 51-72; John Rooney, Jr., "The Pigskin Cult and Other Sunbelt Sports," *American Demographics*, 8 (September 1986): 38-43; Rooney, Zelinsky, and Louder, *Remarkable Continent*, 215, 244, 272-74. Popular acceptance of "Sunbelt" as a new regional term is another example of regional patterning that spans traditional boundaries. See Richard Bernard and Bradley Rice, eds., *Sunbelt Cities: Politics and Growth since World War II* (Austin, Tex., 1983), 1-30; and Carl Abbott, "New West, New South, New Region: The Discovery of the Sunbelt," in Raymond Mohl, ed., *Searching for the Sunbelt* (Knoxville, Tenn., 1990).

²⁰ John Shelton Reed, *The Enduring South* (Lexington, Mass., 1972); "Fifteen Southerners," *Why the South Will Survive* (Athens, Ga., 1981); Louis D. Rubin, ed., *The Lasting South* (Chicago, 1957); Carl Degler, *Place over Time: The Continuity of Southern Distinctiveness* (Baton Rouge, La., 1977). Also see U. B. Phillips, "The Central Theme of Southern History," *AHR*, 34 (October 1928): 30-43; John Shelton Reed, *Southerners: The Social Psychology of Sectionalism* (Chapel Hill, N.C., 1983); David R. Goldfield, *Cottonfields and Skyscrapers: Southern City and Region, 1607-1980* (Baton Rouge, 1982); Charles Roland, "The Ever-Vanishing South," *Journal of Southern History*, 48 (February 1982): 3-20; and Carl Degler, "Thesis, Antithesis, Synthesis: The South, the North, and the Nation," *Journal of Southern History*, 53 (February 1987): 12-13. Zelinsky's classic article "Where the South Begins" adduces historical data to answer a question posed in the present tense.

summary essay, "lingered until after World War II," when Washington began to emerge as a "world capital rather than a southern city." Raymond Gastil found that Washington's "southern quality" survived at least to the mid-1970s. In assigning the city to the South, he acknowledged its diverse population and cosmopolitan ties but found nevertheless that it retained a southern ambiance compounded of race, language, and climate.²¹

The early articulation of American cultural regions was followed in the nineteenth century by the emergence of a second inclusive regional pattern through the definition and development of a northeastern industrial core that centralized economic control over peripheral resource regions. The result is a dual regional patterning on a continental scale. The evolution of the American core includes four stages: the differential growth of the Atlantic ports and the emergence of New York, Philadelphia, and Boston as national economic centers in the eighteenth and early nineteenth centuries; the attachment of a growing system of midwestern cities to these eastern centers between 1820 and 1870; the concentration of American industry in the core states through the reciprocal growth of markets and manufacturing between 1850 and 1920; and the twentieth-century concentration of control functions in the cities of the Northeast and Old Northwest.²² By 1950, the industrialized corridor that extended from Baltimore and Boston to Milwaukee and St. Louis contained 7 percent of the nation's land area but 43 percent of its population, 50 percent of its income, and 70 percent of its manufacturing employment. Flows of investment, trade, business and scientific information, and the expanding reach of national institutions have allowed the values and behaviors of the core to permeate the entire United States as its "national" culture.²³ Within the same model, the American South and West have been relegated to the nation's economic periphery, albeit with bitter complaints about the plundered colonial status of the West, the rise of a self-conscious southern regionalism, and public attention to southern backwardness.²⁴

Analysis of changing relations between core and periphery has been the province of scholars who stress the economic base of human activities and institutions. An older tradition of economic geography defines regions in terms of homogeneity of economic base and nodal organization of trade and communication. The descrip-

²¹ Gutheim, *Potomac*, 277; Constance McLaughlin Green, *Washington: Village and Capital, Washington: Capital City, 1878-1950* (Princeton, N.J., 1963); and "Washington, D.C.," in David C. Roller and Robert W. Twyman, eds., *The Encyclopedia of Southern History* (Baton Rouge, La., 1979), 1311.

²² Jacob Price, "Economic Function and the Growth of American Port Towns in the Eighteenth Century," in Donald Fleming and Bernard Bailyn, eds., *Perspectives in American History: III* (Cambridge, Mass., 1974); James E. Vance, *The Merchant's World: The Geography of Wholesaling* (Englewood Cliffs, N.J., 1970), 68-79; Robert G. Albion, *The Rise of New York Port, 1815-1860* (New York, 1939); David Gilchrist, ed., *The Growth of the Seaport Cities, 1790-1825* (Charlottesville, Va., 1967); James Livingood, *The Philadelphia-Baltimore Trade Rivalry, 1790-1840* (Harrisburg, Pa., 1947); Allan Pred, *Urban Growth and the Circulation of Information: The United States System of Cities, 1790-1840* (Cambridge, Mass., 1973); and *Urban Growth and City Systems in the United States, 1840-1860* (Cambridge, Mass., 1980); A. L. Kohlmeier, *The Old Northwest as the Keystone in the Arch of American Federal Union* (Bloomington, Ind., 1938); John M. Clark, *The Grain Trade in the Old Northwest* (Urbana, Ill., 1966); David Ward, *Cities and Immigrants* (New York, 1971), 11-49; David R. Meyer, "Emergence of the American Manufacturing Belt: An Interpretation," *Journal of Historical Geography*, 9 (1983): 145-74.

²³ Edward Ullman, "Regional Development and the Geography of Concentration," *Papers and Proceedings of the Regional Science Association*, 4 (1958): 179-98.

²⁴ For summaries of the regional response, see Michael O'Brien, *The Idea of the American South, 1920-1941* (Baltimore, Md., 1979); Gavin Wright, *Old South, New South: Revolutions in the Southern Economy since the Civil War* (New York, 1986); William G. Robbins, "The 'Plundered Province' Thesis and the Historiography of the American West," *Pacific Historical Review*, 55 (November 1986): 577-98; and Gene Gressley, "Regionalism and the Twentieth Century West," in Jerome O. Steffen, ed., *The American West: New Perspectives, New Dimensions* (Norman, Okla., 1979), 197-234.

tion of the northeastern Megalopolis by French geographer Jean Gottmann is the climax of the traditional approach.²⁵ The shared economic characteristic of the urbanized northeastern seaboard is the concentration of tertiary activities. At the same time, northeastern cities are the "hinges" that control and connect the resources of the United States to the Atlantic world. *Megalopolis* is thus an updated version of the "metropolitan thesis" with which Canadian historians have interpreted continental growth in terms of the interactions of successive resource regions and metropolitan gateways.²⁶

More recently, writers working within the fields of historical sociology, regional planning, and political economy have approached the core in terms of the changing international division of labor and its impact on regional functioning over the last two decades. They draw on world-systems theory in which the "metropolis" is expanded from a commercial capital to a dominant economic macro-region controlled by one or several nations. Regional patterning is seen as an expression of economic power or control-dependency relations. National regions nest within and frequently repeat the structures of a bifurcated world economy whose division between core and periphery explains the spatial unevenness of economic development and its exploitative consequences. The historical experience of the northeastern United States illustrates the evolution of a European colonial periphery into a new core region with its own periphery, followed by the rise of competing cores in the American Southwest and western Pacific.²⁷

Even more than cultural analysts, these specialists are satisfied with the specific categories of information that are privileged by their theories. Because of the timing of economic differentiation, the writers show substantial interest in tracing developments over the last 125 years, but some also tend to utilize historical data for illustrating rather than testing a priori theories. Practitioners in this tradition may recognize cultural regions but tend to diminish their importance. Ann Markusen, for example, offered an explicitly economic definition in which "what distinguishes regions and subregions from each other are the relatively unique sectoral configurations of their local economies." Richard Bensil stated a similar position: "[A]lthough expressed in cultural or religious terms at times, the historical alignment of sectional competition in America is primarily a product of the relationship of the separate regional economies to the national political economy and the world system."²⁸

²⁵ Jean Gottmann, *Megalopolis: The Urbanized Northeastern Seaboard of the United States* (New York, 1961).

²⁶ J. M. S. Careless, "Frontierism, Metropolitanism, and Canadian History," *Canadian Historical Review*, 35 (1954): 1-21; Donald Kerr, "Metropolitan Dominance in Canada," in John Warkentin, ed., *Canada: A Geographical Interpretation* (Toronto, 1968), 531-55; Donald F. Davis, "The Metropolitan Theme and the Writing of Canadian Urban History," *Urban History Review*, 14 (October 1985): 95-114.

²⁷ For a variety of ways to frame world development in core-periphery terms, see William H. McNeill, *The Rise of the West: A History of the Human Community* (Chicago, 1963); and *The Great Frontier: Freedom and Hierarchy in Modern Times* (Princeton, N.J., 1983); Andre Gunder Frank, *Capitalism and Underdevelopment in Latin America* (New York, 1969); Immanuel Wallerstein, *The Modern World System: Capitalist Agriculture and the European Economy in the Sixteenth Century* (New York, 1974); *The Capitalist World Economy* (New York, 1979); *The Modern World-System, Vol. 2: Mercantilism and the Consolidation of the European World Economy* (Orlando, Fla., 1980); and *The Politics of the World Economy* (New York, 1984). Alejandro Portes, "On the Sociology of National Development: Theories and Issues," *American Journal of Sociology*, 82 (July 1976): 55-85; Bernard Bailyn, "The Challenge of Modern Historiography," *AHR*, 87 (February 1982): 1-24; and Bailyn, *The Peopling of British North America: An Introduction* (New York, 1986); Peter J. Hugill, "Structural Change in the Core Region of the World-Economy, 1830-1945," *Journal of Historical Geography*, 14 (April 1988): 111-27.

²⁸ Ann Roell Markusen, "Industrial Restructuring and Regional Politics," in Robert Beauregard, ed., *Economic Restructuring and Political Response* (Newbury Park, Calif., 1989), 116; Richard F. Bensil,

Economically based studies of Washington have usually described a unitary process of incorporation into the core. Calvin Beale and Donald Bogue's standard inventory of *Economic Areas of the United States* included Washington as an emerging component of the "Atlantic metropolitan belt." Gottmann's *Megalopolis* used data on population density and communication to trace the coalescence of an urbanized region around the organizing center of New York. He placed the northern incorporation of Washington to the years between 1915 and 1930, as a product of changing employment base and the resulting intensification of information exchange with the core. In what might be called "the view from New York," southern traits are curious survivals rather than viable alternatives to the ways of the national core.²⁹

THE ANALYSIS OF REGIONAL CHANGE in Washington begins with the Civil War. The war tilted the balance of federal employment toward the capital. An increase of 4,000 government jobs during the 1860s doubled Washington's share of the national total. The city's federal establishment continued to grow with the evolution of the administrative state through such watersheds as the Pendleton Civil Service Act (1884) and the creation of the Interstate Commerce Commission (1887). The ratio of federal jobs to local employment doubled between 1861 and 1881, when 13,124 federal employees represented 20 percent of all District of Columbia workers (see Table 1). The number of new federal jobs for each decade as a proportion of civilian employment in the District at the start of the decade was higher for the 1860s, 1870s, and 1880s than for any succeeding decade except the 1910s, 1930s, and 1940s (see Table 2).

The war also altered the private city. Substantial capital flowed south in 1861, opening the way for new wealth. Merchants with long-range business faced interrupted access to many of their usual southern customers but found new markets within a booming city and federal establishment. A rapidly rotating population of soldiers and war contractors raised the District of Columbia from 75,000 to 132,000 residents. The end of the war brought 23,000 new black residents from tidewater Maryland and eastern Virginia. A local census in 1867 found that more than two-thirds of black Washingtonians and half of all white residents had arrived since 1860.³⁰

Many of the new white Washingtonians were part of a "carpetbagger generation" of northerners looking for opportunities in a changing southern city. Political appointments were in the gift of a Republican party with its base in the North. Entrepreneurs arrived from the North during the 1860s and 1870s with an eye to

Sectionalism and American Political Development, 1880-1980 (Madison, Wis., 1984), 6. In another essay, "Regionalism and the Capitalist State," in Pierre Clavel, John Forester, and William W. Goldsmith, eds., *Urban and Regional Planning in an Age of Austerity* (New York, 1980), 39, Markusen stated that "there is no major cultural content to contemporary U.S. regionalism. While cultural and ethnic ties may be extremely important at the family and neighborhood levels, they have been largely eliminated on the regional level by capitalist mobility and cultural control." Also see Ann Markusen, *Regions: The Economics and Politics of Territory* (Totowa, N.J., 1987).

²⁹ Gottmann, *Megalopolis*, 181-84; Donald Bogue and Calvin Beale, *Economic Areas of the United States* (Glencoe, Ill., 1961); James Wreford Watson, *North America: Its Countries and Regions* (London, 1968).

³⁰ Green, *Village and Capital*, 263, 293; Gutheim, *Potomac*, 275; Margaret Leech, *Reveille in Washington, 1860-1865* (New York, 1941); "Census of the District of Columbia [1867]," in U.S. Office of Education, *Special Report on the Condition and Improvement of Public Schools in the District of Columbia* (Washington, 1871), 28-38.

TABLE 1
Washington: Federal Civilian Employment in Relation to Total Employment

	<i>Total Employment</i>	<i>Federal Civilian Employment</i>	
1860/61*	24,243	2,199	9.1%
1870/71	49,041	6,222	12.7
1880/81	66,624	13,124	19.7
1890/91	101,119	20,834	20.6
1900/01	126,941	28,044	22.1
1910	157,961	38,911	24.6
1920	236,027	94,110	39.9
1930	243,853	73,032	29.9
1940	319,317	139,770	43.8
1950	620,896	223,312	36.0
1960	791,921	239,873	30.3
1970	1,178,990	327,364	27.8
1980	1,530,954	346,000	22.6

1850–1940: District of Columbia

1950–1980: Metropolitan Area

*Through 1900/01, the total employment figures represent the first year and the federal employment figures the second year.

SOURCES: Federal employment from *Historical Statistics of the United States* (Washington, 1975), and U.S. Office of Personnel Management, *Federal Civilian Workforce Statistics: Annual Report by Geographic Areas* (Washington, 1980).

opportunities in a rapidly changing economy. The result of these changes was to set in motion a dynamic of “reconstruction” and “redemption” that left Washington in the early twentieth century a persistently southern city with new northern connections.

Regional tensions were seen first in the career of Alexander Shepherd, a Washington-born businessman who tried to lead a restructuring of economy and politics after the model of northeastern cities. Shepherd helped organize a short-lived board of trade (1865–1871) that hoped to secure Washington’s commercial future with direct rail links to New York and the Middle West.³¹ He also dominated Washington’s three-year experiment with territorial government as president of the Board of Public Works (1871–1873) and governor (1873–1874). His program of public investment on roads, bridges, sewers, and water lines was intended to give Washington an up-to-date infrastructure comparable to New York’s or Philadelphia’s. The goal was clear to supporter George Townsend, who described the value of the public works projects and complained that Washington before Shepherd “had always been a Southernized city, little intent upon things of general value, and immethodical and slovenly as to its police, sanitary, and scientific regulations.”³²

As historian William Maury has detailed, the program of creating a modern, business-oriented city took a middle path between radical Republicans who saw Washington as a testing ground for racial progress and wealthy, southern-oriented

³¹ Elizabeth Miller, “The Washington Business Community in the Nineteenth Century: Dreams and Disappointments,” paper delivered at D.C. Historical Studies Conference, 1980, in Columbia Historical Society Library, Washington, D.C.; Harvey W. Crew, ed., *Centennial History of Washington, D.C.* (Dayton, Ohio, 1892), 413; *Report of the Joint Committee on Manufacturers of the Legislative Assembly of the District of Columbia* (Washington, 1872), 22.

³² Townsend, *New Washington*, 7.

TABLE 2
Washington: Growth of Federal Civilian Employment in Relation to Total Employment

	<i>Change in Federal Jobs</i>	<i>Change as Percent Beginning Total Employment</i>
1861-71	4,023	16.6%
1871-81	6,902	14.1
1881-91	7,710	11.6
1891-1901	7,760	7.7
1901-10	10,867	8.6
1910-20	55,199	34.9
1920-30	-21,078	-8.9
1930-40	66,738	27.4
1940-50	83,542	26.2
1950-60	16,561	2.7
1960-70	87,496	11.0
1970-80	18,631	1.6

For the first five decades, the beginning total employment is taken from the decennial census year. These base figures through 1940 are for the District of Columbia; for 1950-70 for the Washington Metropolitan Area.

SOURCES: Federal employment from *Historical Statistics of the United States* (Washington, 1975), and U.S. Office of Personnel Management, *Federal Civilian Workforce Statistics: Annual Report by Geographic Areas* (Washington, 1980).

members of Washington's antebellum business community. By offering very specific benefits to politicians and investors from the North, however, Shepherd's administration opened itself to damaging congressional investigations. In 1874, an increasingly conservative Congress abolished territorial government and substituted a system in which a three-member commission appointed by the president supervised the expenditure of carefully guarded congressional appropriations. Maury's conclusions summarize the conflicting regional policies at issue in the early 1870s:

Isolated from the left of their own party, and rebuffed by potent old-line Washingtonians, the Republicans, many of whom were relatively new to the city, sought to solidify their control. They used the techniques of northern machine politics, but like Republicans in other southern cities they were opposed by ante-bellum Democrats. The city government became an island of Grant Republicanism set about by a boiling sea of opposition. As the early shoots of Bourbon recovery were coming up in other parts of the South, its full flower became visible in Washington. By mid-1874, reconstruction in Washington had ended.³³

The failures of Shepherd's efforts to make Washington a part of the emerging national core prefigured the regional dynamic that operated over the longer period from the 1880s to 1920s. Indeed, the 1880s marked a high tide of incorporation into the world of the northeastern social elite. With an eye to leisured society, one journalist in 1881 declared Washington to be an annex of New York, its "winter end . . . as Newport is the summer extension of the metropolis."³⁴ The attenuation of these same social connections, however, was anticipated in Henry Adams's

³³ William M. Maury, *Alexander 'Boss' Shepherd and the Board of Public Works* (Washington, 1975), 50.

³⁴ George Lathrop, "A Nation in a Nutshell," *Harper's New Monthly Magazine*, 62 (March 1881): 542; Julian Ralph, *Dixie, or Southern Scenes and Sketches* (New York, 1896), 372.

Democracy (1880). Madeleine Lee of New York, the novel's protagonist and perceptual filter, is a visitor who descends on Washington out of boredom and leaves after a few social seasons. One of the rivals for her attention is John Carrington, a lawyer from southside Virginia transplanted to Washington to repair his fortunes after the war. Carrington arrives in Washington before Madeleine Lee, fits in more easily with local manners, and stays after she has departed. Similarly, the most striking characteristic of Washington from the 1890s through the 1920s is the reassertion of southern connections. Despite the continued presence of successive political generations of northerners and the expansion of national institutions, Washington in the Coolidge years was as closely tied to the South as in the years of Rutherford B. Hayes and Madeleine Lee's fictionalized social whirl.

Census figures on the place of birth of District of Columbia residents are available for each decade through 1930.³⁵ Native-born whites can be divided among those born in the North and West; those born in the District of Columbia, Maryland, and Delaware; and those born in the remainder of the South. Between 1870 and 1930, the percentage of residents born in the District, Maryland, and Delaware declined from greater than two-thirds to just over one-half (see Table 3). The proportion of southerners more than doubled, but that of northerners grew by only 28 percent. The ratio of northern-born to southern-born whites declined from a high of 1.91 in 1870 to 1.20 in 1930.

The composition of the white elite reflected the same fading influence of the "carpetbagger" generation. The founders of the Washington Board of Trade included the publishers of the *Star* and *Post*, prominent retailers, manufacturers, bankers, and attorneys. Washingtonians soon accepted that the Board of Trade spoke for the civic elite on issues of economic development, government, and public services.³⁶ Half of its founding directors in 1889 were northerners, whose median arrival date in Washington was 1872 (see Table 4). In the 1910s, 1920s, and 1930s, however, native Washingtonians and southerners held an increasing edge as Board of Trade directors. The Chamber of Commerce (1908–1934) defined its goals more narrowly as a "Greater Commercial Washington." Its membership overlapped the Board of Trade but had a heavier representation of smaller retailers and wholesalers. By the 1920s, its leadership too was weighted toward the South.³⁷

³⁵ Ninth Census of the United States (1870), Vol. I: *Statistics of Population*, table 6; Tenth Census of the United States (1880), Vol. 1: *Population*, tables 11 and 12; Eleventh Census of the United States (1890), Vol. 1, Part 1: *Statistics of Population*, tables 25, 28; Twelfth Census of the United States (1900), Vol. 1: *Statistics of Population*, tables 26, 29; Thirteenth Census of the United States (1910), Vol. 1: *Population*, tables 36, 37; Fourteenth Census of the United States (1920), Vol. 2: *Population*, tables 18, 19; Fifteenth Census of the United States (1930), *Population*, Vol. 2, tables 22, 23.

³⁶ Washington Board of Trade, Twenty-Seventh Annual Report (1917–18), 12–13; Washington Chamber of Commerce, First Annual Report (January 14, 1908), 3; *The Book of Washington*, Sponsored by the Washington Board of Trade (Washington, 1930), 3, 7, 250; Green, *Capital City*, 30, 437; Haynes Johnson, *Dusk at the Mountain: The Negro, the Nation, and the Capital* (Garden City, N.Y., 1963), 211–12.

³⁷ Information on the origins of leaders for the Washington Board of Trade and Washington Chamber of Commerce to 1940 was drawn from the obituary and biography files, Washingtoniana Collection, Martin Luther King, Jr. Memorial Library, Washington, D.C., and from the following biographical sources: John P. Coffin, *Washington: Historical Sketches of the Capital City of Our Country* (Washington, 1877); *Leading Merchants and Manufacturers of the City of Washington: A Resume of Trade, Enterprise, and Development* (New York, 1887); *Eminent and Representative Men of Virginia and the District of Columbia of the Nineteenth Century* (Madison, Wis., 1893); A. K. Parris and W. A. Means, eds., *Investor's Handbook of Washington Securities* (Washington, 1901); Allan B. Slauson, ed., *History of the City of Washington: Its Men and Institutions* (Washington, 1903); *District of Columbia: Concise Biographies of Its Prominent and Representative Contemporary Citizens and Valuable Statistical Data* (Washington, 1908); Albert D. Miller, *Distinguished Residents of Washington, D.C.: Science-Art-Industry* (Washington, 1916); *Prominent*

TABLE 3
Place of Birth of District of Columbia Residents: 1870–1930

Native Whites						
	<i>Total</i>	<i>North</i>	<i>Dist of Columbia</i>	<i>Md-Del</i>	<i>Va-W. Va</i>	<i>Other South</i>
1870	72,091	20.8%	53.9%	14.3%	9.4%	1.5%
1880	101,024	20.4%	55.4%	12.5%	9.3%	2.5%
1890	134,836	21.6%	52.6%	12.8%	10.2%	2.8%
1900	175,040	24.3%	47.7%	12.6%	11.5%	3.9%
1910	209,289	23.4%	47.0%	12.7%	12.7%	4.2%
1920	295,299	30.6%	38.4%	11.0%	12.8%	7.1%
1930	320,940	26.7%	40.0%	11.0%	14.6%	7.7%
Native Blacks						
	<i>Total</i>	<i>North</i>	<i>Dist of Columbia</i>	<i>Md-Del</i>	<i>Va-W. Va</i>	<i>Other South</i>
1870	43,324	1.3%	31.0%	27.1%	38.7%	1.9%
1880	59,478	1.7%	41.7%	20.6%	33.8%	2.3%
1890	74,883	1.9%	42.3%	20.1%	32.3%	3.2%
1900	86,446	2.1%	42.0%	18.3%	32.7%	4.9%
1910	93,517	2.7%	43.3%	16.8%	30.5%	6.7%
1920	108,879	3.4%	42.8%	16.0%	27.1%	10.6%
1930	130,450	3.8%	40.2%	12.6%	23.7%	19.7%

SOURCES: Ninth Census of the United States (1870), Vol. 1: *Statistics of Population*, table 6; Tenth Census of the United States (1880), Vol. 1: *Population*, tables 11 and 12; Eleventh Census of the United States (1890), Vol. 1, Part 1: *Statistics of Population*, tables 25 and 28; Twelfth Census of the United States (1900), Vol. 1: *Statistics of Population*, tables 26 and 29; Thirteenth Census of the United States (1910), Vol. 1: *Population*, tables 36 and 37; Fourteenth Census of the United States (1920), Vol. 2: *Population*, tables 18 and 19; Fifteenth Census of the United States (1930), *Population*, Vol. 2, tables 22 and 23.

Washington's changing business leadership showed a special interest in a southern economic strategy between 1895 and 1915. Turning away from the northern markets of the Shepherd years, Washingtonians echoed the rhetoric of the New South, arguing that their city would grow in step with "the progress of the rejuvenated South." Newspapers in the early twentieth century cited lists of Washington entrepreneurs whose trade extended into Virginia, the Carolinas, Georgia, Tennessee, and points beyond. The Southern Railway emitted a stream of regional booster literature from its Washington headquarters. A "trade-getting train" chartered by the *Washington Star* in 1906 hauled a contingent of entrepreneurs along with two exhibit cars through Lynchburg, Roanoke, Raleigh, and other South Atlantic towns.³⁸

Personages of the Nation's Capital (Washington, 1924); John Clagett Proctor, ed., *Washington Past and Present: A History* (New York, 1930); *Who's Who in the Nation's Capital*, editions of 1921–22, 1923–24, 1926–27, 1929–30, 1934–35, and 1939–40.

³⁸ Alexander D. Anderson, *Greater Washington: The Nation's City Viewed from the Material Standpoint* (Washington, 1897); Southern Railway Company, Seventeenth Annual Report (1910–11), 8, Library of Congress, Washington, D.C.; Fairfax Harrison, *The South and the Southern Railway: The Statement of a Record and of an Ambition* (Washington, 1916), 15; *The Southern Field*, 9 (May 1904): 6; Frank Presbrey, *The Southland* (Washington, 1898); *Washington Enterprise*, September 8, 1906; *The Southern Commercial*, October 1, 1906; "Prosperous Washington," *Washington Post* (June 11, 1912), 55; Washington Board of Trade, Fifteenth Annual Report (1905), 50. More detail on changing economic strategies can be found

TABLE 4
Place of Birth of Washington Business Leaders: 1890–1940

	North	Washington	South	Overseas	Not Available
<i>Board of Trade</i>					
<i>Directors</i>					
1890	11	3	5	3	9
1900	15	4	3	3	5
<i>Board of Trade</i>					
<i>Directors and</i>					
<i>Committee Chairs</i>					
1910	12	17	7	2	7
1920	12	21	7	1	5
1930	8	20	11	1	10
1940	13	13	10	1	5
<i>Chamber of Commerce</i>					
<i>Directors</i>					
1920	8	12	8	3	4
1930	10	9	9	3	9

SOURCES: See note 37.

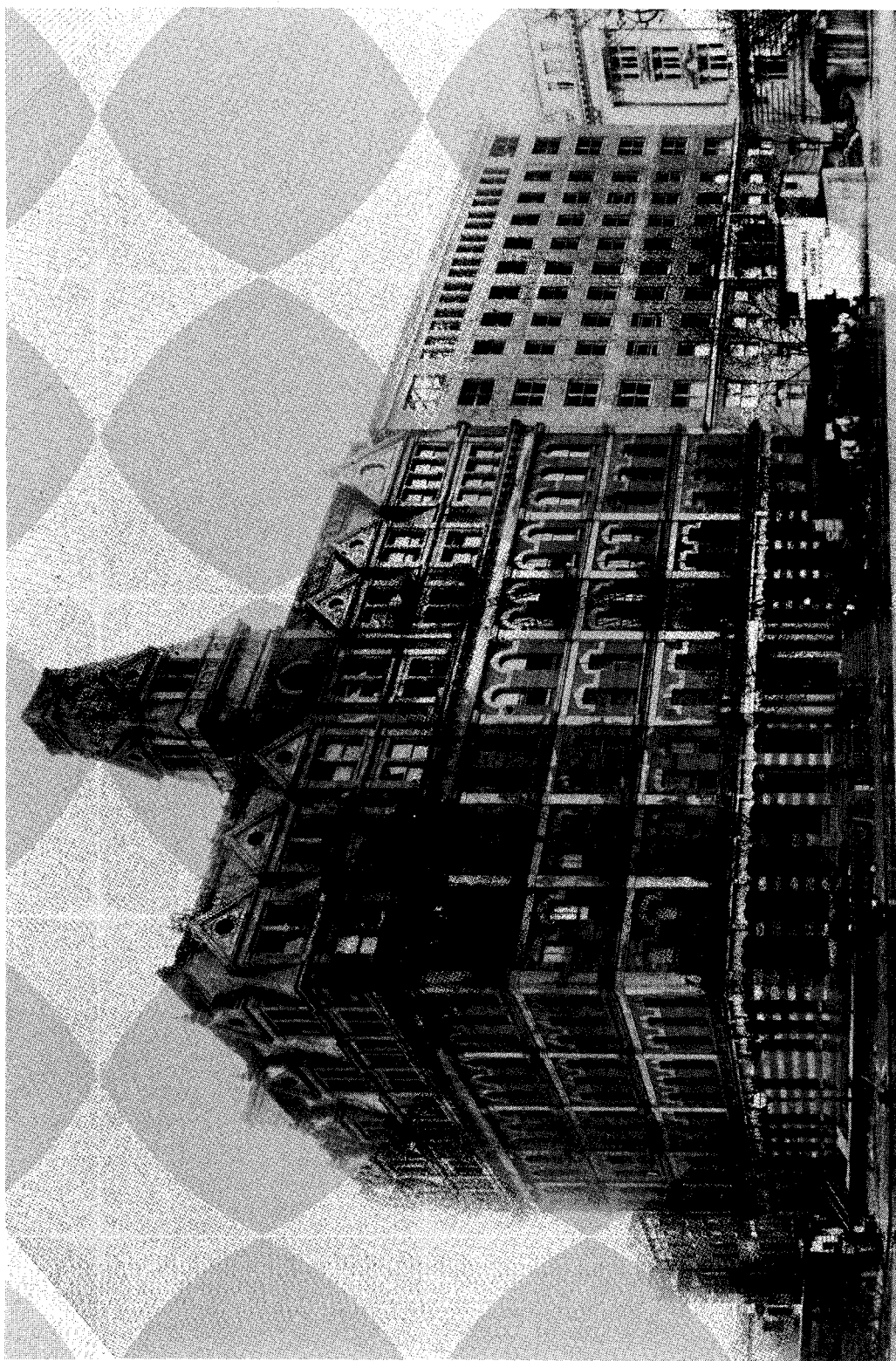
Advocates phrased the southern strategy in the language of geographic destiny. The city was the natural "Gateway to the South" for northern trade and travel. It needed only to develop its wholesaling and banking services to intercept southern customers who might otherwise travel to Baltimore or New York. In addition, Washington was presumably the most convenient point of assembly and processing for the coal, cotton, lumber, iron, and tobacco of the South. Its factories could turn these raw materials into finished goods and sell them back to southern customers through a developed commercial network. Articulated early in the century, these themes remained standard in public discussion through the 1920s.³⁹

Washington's intellectual life also retained a strong southern orientation. Efforts during the later nineteenth century to make Washington a national center for science, education, and the arts are well known. Elements include the founding of "national" Roman Catholic and Methodist universities, the expansion of federal science agencies, and the coalescence of self-conscious scientific and literary circles.⁴⁰ Balancing these efforts, however, were more narrowly regional endeavors.

in Carl Abbott, "Perspectives on Urban Economic Planning: The Case of Washington, D.C., since 1880," *Public Historian*, 11 (Spring 1989): 5–21.

³⁹ Louis P. Shoemaker, *Manufacturing in the District of Columbia and Its Influence on the United States, Copied from the Evening Star and Reprinted by the Business Men's Association* (Washington, 1905); George H. Gall, *Washington: Industrial, Commercial and Civic Features* (Washington, 1908); *Washington Condensed: Five Thousand Facts for Ready Reference* (Washington, 1909); *Industrial Survey of the Washington Metropolitan Area* (Washington, 1928); Washington Chamber of Commerce, *Greater Washington* [monthly magazine], 1920–33; Washington Board of Trade, Tenth Annual Report (1900), 57–58, Eleventh Annual Report (1901), 53, and Twenty-Fifth Annual Report (1915–16), 7–8; Washington Board of Trade, President's Reports for 1936–37 and 1950–51, in Washington Board of Trade Papers, George Washington University.

⁴⁰ James K. Flack, *Desideratum in Washington: The Intellectual Community in the Capital City, 1870–1900* (Cambridge, Mass., 1975); John Tracy Ellis, *The Formative Years of the Catholic University of America* (Washington, 1946); Wilcomb E. Washburn, *The Cosmos Club of Washington: A Centennial History* (Washington, 1978); Robert V. Bruce, *The Launching of Modern American Science, 1946–76* (New York, 1987), 343.



Southern Railway Building, Pennsylvania Avenue and 13th Street NW, Washington, D.C. Formed in 1894, the Southern Railway Company consolidated several smaller railroad lines that tied Washington to the southeastern states. The new company took over the downtown Washington offices previously used by the Richmond and Danville Railroad, one of its component lines. After enlargement in 1899, the Southern Railway headquarters building was a landmark along Pennsylvania Avenue and gave visible expression to Washington's commercial ambitions in the South. This view dates from the 1910s. Courtesy of the Norfolk Southern Corporation archives, Atlanta, Georgia.

The genteel Virginian Thomas Nelson Page made his Washington home a social and literary center for southern writers in the 1890s and 1910s. Southerners residing in Washington founded a Southern History Association in 1896 and sponsored occasional publications and meetings until 1907.⁴¹ The few (and mostly short-lived) Washington periodicals that claimed a regional rather than a national audience offered titles such as *Southern Churchman*, *South Atlantic*, *Southern Guide*, *Southern Literary Messenger*, and *Southern Commercial*.

Post-bellum Washington was especially prominent as a cultural and intellectual center for black Americans. Republican presidents through William Howard Taft reserved a number of federal appointments for blacks and maintained the federal civil service as an available career route. Howard University was explicitly national in scope but drew approximately two-thirds of its students from the Washington area or points south. For black intellectuals around the turn of the century, an appointment to the Howard faculty was often the climax of a career that included previous positions within the system of all-black southern colleges.⁴² Literary circles such as the Saturday Nighters, who met at the home of Georgia Douglas Johnson, brought together native Washingtonians with aspiring writers from the South and Southwest.⁴³

The origins of the broader business and professional leadership of black Washington can be analyzed from biographical compendia published in 1915, 1928, and 1950.⁴⁴ Through the first half of the twentieth century, community leaders remained overwhelmingly local or southern in birth. The proportion of prominent civil servants, educators, clergymen, physicians, real estate dealers, and other business proprietors born in Maryland, Virginia, and the District of Columbia held at a steady 36 to 37 percent between 1915 and 1950; the proportion from the rest of the South ranged between 40 and 46 percent; and northerners constituted a stable minority of 15 to 17 percent.

Although turn-of-the-century Washington remained an arena of opportunity for black professionals from the North, the black community in aggregate was transformed by a rapid increase in the migrants from the South Atlantic states after 1910. From 1880 to 1930, approximately 40 percent of the District's black residents were born within its boundaries (Table 3). The deeper South replaced Virginia and Maryland, however, as the source area for new residents after the Civil War displacement. Net black migration from the Carolinas, Georgia, and Florida was three times that from the North in the 1910s and more than ten times during the 1920s. The ratio of northern-born blacks to those born south of Virginia dropped from .68 in 1870 to .19 in 1930.

What many of Washington's new black migrants found was no longer the

⁴¹ Federal Writers Project, *Washington*, 16; E. Merton Coulter, "What Has the South Done about Its History," in George B. Tindall, ed., *The Pursuit of Southern History: Presidential Addresses of the Southern Historical Association, 1935-63* (Baton Rouge, La., 1964), 20-21; Wilhelmus Bogart Bryan, *Bibliography of the District of Columbia* (Washington, 1898).

⁴² Rayford Logan, *Howard University: The First Hundred Years, 1867-1967* (New York, 1969); Carroll D. Wright, "The Economic Development of Washington," *Proceedings of the Washington Academy of Sciences*, 1 (1899): 183; Constance McLaughlin Green, *The Secret City: A History of Race Relations in the Nation's Capital* (Princeton, N.J., 1967), 207-10; Willard B. Gatewood, Jr., "Aristocrats of Color: South and North, the Black Elite, 1880-1920," *Journal of Southern History*, 54 (February 1988): 3-20.

⁴³ Ronald M. Johnson, "Those Who Stayed: Washington's Black Writers of the 1920s," *Records of the Columbia Historical Society*, 50 (1980): 484-99.

⁴⁴ Comments on career tracks are based on biographical sketches in Frank Lincoln Mather, *Who's Who of the Colored Race: A General Dictionary of Men and Women of African Descent* (Chicago, 1915); Thomas Yesner, *Who's Who in Colored America* (Brooklyn, N.Y., 1928); and G. James Fleming and Christian E. Burckel, eds., *Who's Who in Colored America, 1950* (Yonkers-on-Hudson, N.Y., 1950).

relatively open and "northern" community of the 1870s and 1880s. Instead, the city joined the developing southern system of race relations after 1900. De facto segregation appeared in most public facilities during the early 1900s, at the same time that southern congressmen began to press for formal segregation. The initially integrated Board of Trade became all white. Woodrow Wilson curtailed the customary appointments of black officials in the federal service after 1913 and formally segregated federal offices. By the 1920s, racial relations reached what Constance Green called the "new nadir," when "the national government took the tone of the Deep South, and white people as private persons now looked upon Negroes scarcely as citizens at all."⁴⁵

The imposition of Jim Crow manifested a southern character already noted by many observers. As Henry James remarked after a journey southward in 1906, Washington lay in that zone where "the North ceases to insist, [and] the South may begin to presume." To many northerners, turn-of-the-century Washington remained part of the South. It was on the list of historic southern towns in one book, was called "a Southern city geographically" in another. Even a Chicago-based banking magazine agreed that "the people of Washington are largely Southern by birth and training. Indeed the Capital may well be accounted a Southern City."⁴⁶

This evidence on business orientation, population, public values, and public image suggests that Washington's surge of economic growth and social change during and after the Civil War triggered a two-way process of regional change. Descriptions of metropolitan regions often emphasize their role as importers of culture and information, which is presumed to diffuse outward from a center of origin and move down the urban hierarchy.⁴⁷ Because post-bellum Washington was located near the edge of a culturally assertive North and an expanding economic core, it provided a gateway through which new ideas and people entered the South. Many assumed that Washington's national institutions would in fact represent the values of the successful North and the interests of the national core. Washington's southern commercial strategy remained dependent on capital from the larger northeastern cities. The District of Columbia also experienced a slow but persistent shift toward a northern-born population between 1850 and 1930.⁴⁸

Gateways allow travel in two directions, however. Although Washington represented and transmitted a dominant northern culture and interest, it had the reciprocal function of attracting and concentrating southerners and centralizing institutions already oriented to the South. The assembly of small railroad companies into the Southern, Atlantic Coast Line, and Seaboard Air Line rail systems during the 1890s, for example, may have opened southern markets to penetration by northern manufacturers, but the railroads also made Washington an easy destination for southerners looking for urban opportunities. Networks for professional recruitment and the dissemination of ideas spread south as well as north. The same economic processes that tied the city to the North thus stabilized its southern cultural affiliations. The city in 1930 was linked to national networks

⁴⁵ Green, *Secret City*, 155–83, 214; Robert Harrison, "The Ideal of a 'Model City': Federal Social Policy for the District of Columbia, 1905–1910," *Journal of Urban History*, 15 (August 1989): 455–56.

⁴⁶ Henry James, *The American Scene* (London, 1907), 303; Lyman P. Powell, *Historic Towns of the Southern States* (New York, 1900); W. Y. Barnet, "Washington Entertains Bankers," *The Banking and Mercantile World*, 7 (November–December 1905): 206; Ralph, *Dixie*, 341–46.

⁴⁷ John Borchert, "America's Changing Metropolitan Regions," *Annals of the Association of American Geographers*, 62 (June 1972): 352–73; Allan Pred, *City-Systems in Advanced Economies* (New York, 1977).

⁴⁸ Northern-born residents constituted 7 percent of all District of Columbians in 1850, 14 percent in 1870, 15 percent in 1890, 17 percent in 1910, and 20 percent in 1930.

focused on the Northeast, but it was equally attached to the South in the character of everyday life. It was restructured but not reconstructed.

THE EQUILIBRIUM OF EARLY TWENTIETH-CENTURY WASHINGTON survived the Great War but not the Great Depression. Mobilization in 1917 and 1918 tripled the federal civilian work force in the District of Columbia and increased the number of northerners there by 40,000 between 1910 and 1920, but the new factors faded from the Washington scene nearly as fast as they had appeared. The impact of World War I is invisible in employment and population series that skip from 1916 to 1925. Permanent expansion of the federal establishment waited until the 1930s. The net growth in federal jobs for the 1930s equaled 27.4 percent of the District employment at the start of the decade (Table 3). The "pencil-sharpener revolution" made Washington one of the few American cities to experience housing price inflation and rapid suburbanization during the depression decade. Mobilization in 1940 and 1941 brought more of the same. Like San Diego, Mobile, or Norfolk, Washington was a wartime boom town where soldiers, sailors, and defense workers crowded available housing, jammed restaurants and theaters, and wandered the streets on hot summer nights.⁴⁹ Over the full twenty years, federal civilian employment tripled to 223,000 (Table 1).

The result was a city of increased social complexity in which New Dealers, civil servants, and lobbyists brought new ideas about the right way to do things and counterbalanced the established families who had set the city's social tone. Gore Vidal's *Washington, D.C.* (1967) offers a version of the contrast between old and new when his fictional society columnist remarks that "our lovely, gracious Southern city has been engulfed by all these . . . [she casts around for a tactful phrase] . . . charmin' people who've opened our poor eyes to so many things undreamed of in our philosophy."⁵⁰ The black community mirrored some of the tensions among white Washingtonians. A growing salaried class of blacks with steady jobs in the lower levels of the federal service challenged the older District elite. The accumulating proletariat of poorly paid service workers and underemployed laborers tended to push both groups into the background. Regional affiliation since the war has therefore involved the overlapping claims of multiple Washingtons—old and new white, old and new black.

Recent Washington has alternated decades of slower growth in the 1950s and 1970s with further expansion during the 1960s and 1980s. The addition of 88,000 federal jobs in the 1960s stands out in the postwar decades, but the increase had relatively less impact than the civil service boom of 1930–1950. Activities ancillary to the national government rather than direct employment fueled renewed growth in the 1980s. Examples include tourism, federally funded research and development, expanding sales and engineering staffs for defense contractors, and more than 2,000 trade and professional associations.⁵¹ Aggregate metropolitan popula-

⁴⁹ John Dos Passos, *State of the Nation* (Boston, 1944); Scott Hart, *Washington at War, 1941–45* (Englewood Cliffs, N.J., 1970); Alden Stevens, "Washington: Blight on Democracy," *Harper's Magazine*, 184 (December 1941): 50–58; Brinkley, *Washington Goes to War*; Bureau of Business and Economic Research, University of Maryland, *Metropolitan Washington after 150 Years* (College Park, Md., 1950).

⁵⁰ Gore Vidal, *Washington, D.C.* (Boston, 1967), 251.

⁵¹ Gail Garfield Schwartz, *Technology Oriented Firms in the Washington Area* (Washington, 1984); Edward J. Malecki, "Science and Technology in the American Metropolitan System," in Stanley Brunn and James O. Wheeler, eds., *The American Metropolitan System: Present and Future* (New York, 1980), 134–40;

tion, as measured by a geographically expanding metropolitan district (1930–1940) or metropolitan area (1950–1985) grew from 621,000 in 1930 to 1,464,000 in 1950, 2,861,000 in 1970, and 3,496,000 in 1985.

This exploding metropolis has been increasingly tied into the dense communication network of the northeastern seaboard. Gottmann's *Megalopolis* (1961) cited data on the density of telephone calls, newspaper circulation, air traffic, and other flows of people and ideas. A decade later, Delbert Miller tested Gottmann's thesis by analyzing networks among local and national elites in the American Northeast. His data described a New York–Washington axis for the interaction of metropolitan leaders and the circulation of an elite concerned with regional and national decisions, although he presented little evidence of Megalopolis as a single regional community.⁵² Black leadership by the 1980s had also begun to tilt northward. Persons born outside the South accounted for 28 percent of entries for the Washington metropolitan area in the 1985 edition of *Who's Who in Black America*.⁵³

The origins of students in Washington area colleges and universities have reflected the same connection. Net migration of students into the District of Columbia jumped from 10,000 before World War II to 23,000 by 1951, stabilized in the 1950s, and jumped again to 37,000 after 1958, making higher education one of Washington's major exports.⁵⁴ A simple gravity model of spatial interaction allows the allocation of total migration for a given year on the assumption that the number of students coming to Washington from each state will be directly proportional to the state's population and inversely proportional to its distance from Washington.⁵⁵ Comparison of predicted and actual values consistently shows the over-representation of New Yorkers, New Englanders, Floridians, and students from west of the Mississippi at District of Columbia colleges and the under-representation of Pennsylvania, the Middle West, and most of the South. The unexpected numbers of students from the West presumably testify to the perceived advantages of education in the national capital. The unexpected numbers of northeasterners, in contrast, suggest that Washington has become part of an educational network that may be an important carrier of northern values.⁵⁶

The most obvious regional impact of Washington's mid-century transformation was a rapid revision of the terms of debate on key public issues. Organized pressure for

"Trade Groups Flock to Washington," *Washington Post*, February 14, 1987; Gladstone Associates, *Visitors and Their Contribution to the Washington Economy* (Washington, 1975).

⁵² Gottmann, *Megalopolis*, 588–606; Delbert Miller, *Leadership and Power in the Bos-Wash Megalopolis* (New York, 1975), 7–20, 87–91, 350–51.

⁵³ *Who's Who in Black America*, 4th edn. (Lake Forest, Ill., 1985).

⁵⁴ George Zook, *The Residence of Students in Universities and Colleges* (Washington, 1922); Frederick J. Kelly and Betty A. Patterson, *Residence and Migration of College Students* (Washington, 1934); Frederick J. Kelly and Ruth E. Eckert, *Residence and Migration of College Students, 1938–39* (Washington, 1945); Robert Strong, *Residence and Migration of College Students, 1949–50* (Washington, 1951); Mabel C. Rice and Paul L. Mason, *Residence and Migration of College Students, Fall 1963: State and Regional Data* (Washington, 1966); George H. Wade, *Residence and Migration of College Students, Fall 1968: Analytic Reports* (Washington, 1968).

⁵⁵ For introductions to the gravity model, see Walter Isard, *Introduction to Regional Science* (Englewood Cliffs, N.J., 1975), 42–50; Gerald A. Carrothers, "Historical Review of the Gravity and Potential Models of Human Interaction," *Journal of the American Institute of Planners*, 22 (1956): 94–102; R. W. Thomas and R. J. Huggett, *Modeling in Geography: A Mathematical Approach* (Totowa, N.J., 1980), 133–52.

⁵⁶ The proportion of Howard University students from the Northeast has gradually but steadily risen from 16 percent in 1945 to 25 percent in 1985, reflecting the participation of northern blacks in the same regional network (data furnished by Howard University registrar, March 5, 1987). Compare the discussion of the backgrounds of Washington's black elite in John J. Harrigan, *Negro Leadership in Washington, D.C.* (Ph.D. dissertation, Georgetown University, 1971).

changes in the city's strict codes of racial segregation appeared in 1947–1948 with an executive order barring racial discrimination in federal employment, reports from the President's Committee on Civil Rights, and findings by the National Committee on Segregation in the Nation's Capital. Catholic schools desegregated in 1950, most private restaurants and theaters in the District between 1951 and 1953, and public schools and parks in 1954. Suburban Maryland counties implemented freedom-of-choice plans for their public schools in 1955, and northern Virginia school systems integrated after the collapse of massive resistance in 1959.⁵⁷

The sense of Washington as part of a distinct southern economy also lessened. The leaders of the Board of Trade since 1950 have included fewer southerners and more northerners, along with a core of second-generation or third-generation Washingtonians. Many of the northerners have been professional managers with national careers and cosmopolitan allegiances.⁵⁸ They have provided a receptive audience for the restatement of the southern commercial strategy in nonregional terms. In particular, key planning documents in 1958 shifted local economic discussion from the language of geographic imperialism to the spatially neutral industrial categories of modern economic analysis.⁵⁹

Incorporation of northern values and connections within Washington's public institutions of government, education, and commercial promotion has been balanced

⁵⁷ The lag between the Maryland and Virginia responses reflects an internal gradient within Washington's suburban ring between Montgomery County, Maryland, and Arlington and Fairfax counties, Virginia. Montgomery County has accommodated much of the Jewish population of the metropolitan area, and its residents tend to maintain close ties to the Northeast. Montgomery County consumers are the most New York-focused and fashion-conscious among suburban Washingtonians. The Virginia suburbs, in contrast, retained residual traces of segregationist values, such as a racially closed housing market, through the 1960s. The presence of the Pentagon has tended to attract migrants (including military retirees) with previous experience of Sunbelt military bases and defense contractors. If Montgomery County reproduces the suburbs of Philadelphia or New York, Fairfax County reproduces the up-to-date urban South of northside Atlanta or north Dallas with an emphasis on the pleasures of unregulated entrepreneurship. Prince Georges County, Maryland, has followed a third path in which the rural South has adapted to the economic and political empowerment of blacks. Historically part of the corn and tobacco-farming Tidewater region, the county saw the most entrenched opposition to racial desegregation. The arrival of middle-class blacks seeking a suburban alternative to the District of Columbia, however, has made it a mixed society in which both races share political power. The differences among the three suburban sectors have been maintained by the limited scale of inter-county migration, especially between Maryland and Virginia. The most thorough studies of intra-suburban differences are Howard Gillette, Jr., "A National Workshop for Urban Policy: The Metropolitanization of Washington, 1946–68," *Public Historian*, 7 (1985): 6–27; and Dennis Gale, *Washington, D.C.: Inner City Revitalization and Minority Suburbanization* (Philadelphia, 1987), 11–49, 184–95. Demographics are treated in Stanley K. Bigman, *The Jewish Population of Greater Washington in 1956* (Washington, 1957); and Eunice Grier and George Grier, "The Sorting Out of Washingtonians: Patterns of Residential Movement in the Metropolitan Area," in Atlee E. Shidler, ed., *Greater Washington in 1980* (Washington, 1980), 49–68. A perceptive popular treatment of social and behavioral differences is Barbara Palmer, "Maryland vs. Virginia," *Washingtonian*, 16 (April 1981): 126–33. Desegregation is treated in Green, *Secret City*, 274–312; Sam P. Wiggins, *The Desegregation Era in Higher Education* (Berkeley, Calif., 1966), 3–6, 18–19; Benjamin Muse, *Virginia's Massive Resistance* (Bloomington, Ind., 1961), 3, 73, 139, 142, 159; George H. Callcott, *Maryland and America, 1940 to 1980* (Baltimore, Md., 1985), 152–54, 244; U.S. Commission on Civil Rights, *A Long Day's Journey into Light: School Desegregation in Prince Georges County* (Washington, 1976), 69–148.

⁵⁸ States of birth of presidents of the Washington Board of Trade from 1940 to 1985 compiled from information in the Board of Trade Papers, George Washington University. Also see Atlee Shidler, "Local Community and National Government," in Shidler, ed., *Greater Washington in 1980*, 24; Robert N. Gray to author, April 23, 1987. Washington thus appears to display the transition from locally oriented to cosmopolitan elites as discussed in Robert K. Merton, *Social Theory and Social Structure*, enlarged edn. (New York, 1968), 441–72.

⁵⁹ Council for Economic and Industry Research, *Economic Base Survey*; Hamer and Company, "Economic Development in the Washington Area"; Metropolitan Washington Board of Trade, "Summary of Activities, 1954–1970," Board of Trade Papers, George Washington University.

by the persistence of southern characteristics and connections in private values and behavior. Even in recent versions of the "big change," Washington commentators of both races have agreed that southern styles of social behavior lasted at least into the 1970s before yielding to Manhattanization.⁶⁰ More people in the Washington area read *Southern Living* than *New York*. In complementary fashion, residents of the South Atlantic states (and the West) are more likely to keep up on developments in the capital by subscribing to *The Washingtonian* than are northeasterners and midwesterners. The conscious decision of owner George Preston Marshall to market the Washington Redskins of the National Football League as an all-white standard bearer for the South during the 1940s and 1950s is an extreme case of the assumption of southern values by a Washington organization.⁶¹

Washington's role in the propagation of indigenous South Atlantic musical traditions also illustrates the intensification of southern cultural ties. The migrations of the 1930s and 1940s brought the evolving bluegrass music of the eastern Appalachians to Washington. As the most accessible and "southern" of the major eastern markets, greater Washington has supported specialized clubs and radio stations since the 1950s, summer festivals and publications since the 1960s. A number of practitioners of the South Atlantic blues style similarly moved to Washington as the first stop "up the road" to the North. Blues artists in the Washington area display styles distinctive of the Virginia and Carolina Piedmont. Gospel ensembles in Washington keep open an active, creative exchange by hosting choirs and quartets from the Carolinas and Georgia and by making their own annual visits to particular South Atlantic churches.⁶²

Professional and amateur musicians are one specialized subgroup within a much larger migration stream that has continued to reinforce Washington's southern character. Aggregate migration data for 1935–1940, 1955–1960, and 1975–1980 show a consistent over-representation from the South Atlantic states, Southwest, and Far West compared with the numbers predicted by a gravity model. Delaware, Pennsylvania, New Jersey, the Ohio Valley, and the Great Lakes states have been under-represented (Figure 1). In the aggregate, the South exclusive of Maryland, Delaware, and Virginia accounted for 32 percent of migrants to the District of Columbia in the later 1930s, for 34 percent of migrants to the larger metropolitan area in the later 1950s, and for 30 percent in the later 1970s.⁶³

Migrants with paychecks from the Department of Defense are especially likely to have had southern connections, for the growth of military administration in Washington has meant the expansion of an institution historically dominated by southerners. Despite declines after both world wars, the postwar American officer corps continued to draw disproportionately from people of southern birth and education. As late as 1980, the South supplied more than its per capita share of all

⁶⁰ Charles N. Concini, "Washington Then and Now," *Washingtonian*, 11 (October 1975): 51–66; Means, "Northernization of Washington," 82–86.

⁶¹ Audit Bureau of Circulation data furnished by the several magazines; James J. Haggerty, *Hail to the Redskins* (Washington, 1974), 102, 128, 134; David Slattery, *The Washington Redskins* (Virginia Beach, Va., 1977), 63.

⁶² Bruce Bastin, *Red River Blues: The Blues Tradition in the Southeast* (Urbana, Ill., 1986), 315–17; Neil V. Rosenberg, *Bluegrass: A History* (Urbana, Ill., 1985), 27, 112–13, 138–39, 147–48, 224–26, 323–30, 354–55; Glenn Hinson to author, May 30, 1987; Michael Licht to author, April 24, 1987.

⁶³ Migration data for 1955–1960 are for state economic areas covering the District of Columbia; Montgomery and Prince Georges counties, Maryland; and Alexandria, Arlington, and Fairfax counties in Virginia. For 1975–1980, data from the Metropolitan Washington Council of Governments and the Northern Virginia Planning District Commission allowed the construction of a metropolitan region including the District of Columbia; Montgomery, Prince Georges, and Charles City counties in Maryland; and Alexandria, Arlington, Fairfax, Prince William, and Loudoun counties in Virginia.

residents of the District of Columbia and Virginia are military retirees (the Virginia figures reflect concentrations in both the Washington and Hampton Roads areas).⁶⁶

A substantially larger portion of Washington's new southerners have been blacks from the Carolinas and Georgia who have maintained both folk culture and family connections. Anthropologist Brett Williams has documented aspects of surviving southern folkways dealing with food, fishing, gardening, and healing. If the airline shuttles are the armatures that support a Washington-Northeast communications network, Interstate 95 from Washington to Savannah provides the basis for an equally active Washington-Southeast social network. Black colleges in the Carolinas have alumni groups in Washington. Black churches in Washington are likely to hold "Carolina Days," and social clubs for migrants from specific South Atlantic communities have survived for decades. Family members visit between Washington and the Carolinas, attend annual reunions, and shift residence back and forth between the South and the city. Many Washington families regularly send their children or grandchildren south for the summer, keeping alive a sense that the South is home as much as the city up the road.⁶⁷

Travel patterns facilitated by postwar highway building have also intensified Washington's connections with nearby rural districts where historic southern culture survived into the later twentieth century in relative isolation from the industrial growth of the New South. Chesapeake Bay and its tidal rivers have long been a local migration field and an economic resource for fishing boats operating out of the lower Potomac. The northern counties of Virginia and the upper Potomac basin similarly looked to Washington as the communication and retail center.⁶⁸ Particularly since the 1960s, recreational choices have brought contemporary Washingtonians into contact with the same areas. Census data on "homes held for occasional use," first available for 1980, identify concentrations of recreational activity in the mountain counties of the upper Potomac drainage, on the Tidewater Virginia peninsulas, the eastern shore of Chesapeake Bay, and the Atlantic coasts of Delaware, Maryland, and North Carolina. Annapolis, Fredericksburg, and Culpeper were incorporated into the Washington exurbs during the 1970s. Speaking for characters with jobs in Washington and Baltimore, novelist John Barth recently commented that "the purpose of Maryland's mainland, where one lives and works, is to yearn toward its Eastern Shore, where one plays and dreams." The yearning seems to include the Sunday edition of the *Washington Post*, which has a 5 percent sales penetration as far to the southeast as Nags Head.⁶⁹

Local opinion leaders have searched in recent years for the right phrase to capture the sense of an emerging subregion. There was brief interest during the 1970s in the idea of Washington as a Sunbelt city as a way to define its independence from the aging Northeast core. Considerably more attention has been given to the promotion of more localized ideas such as a Washington-Baltimore Common Market, a functionally consolidated supermetropolis that can outshine Philadelphia and match Chicago or

⁶⁶ G. Taylor Barnes and Curtis C. Roseman, "The Effect of Military Retirement on Population Redistribution," *Texas Business Review*, 55 (May-June 1981): 100-06.

⁶⁷ Brett Williams, *Upscaling Downtown: Stalled Gentrification in Washington, D.C.* (Ithaca, N.Y., 1988); "The South in the City," *Journal of Popular Culture*, 16 (Winter 1982): 30-41; and interview, April 10, 1987. John Cromartie and Carol Stack, "Reinterpretation of Black Return and Nonreturn Migration to the South, 1975-1980," *Geographical Review*, 79 (July 1989): 297-310.

⁶⁸ Joseph McGee, *Social and Economic Aspects of the Functional Entity of Washington, D.C.* (Washington, 1947); William Warner, *Beautiful Swimmers: Watermen, Crabs, and the Chesapeake Bay* (Boston, 1976).

⁶⁹ John Barth, *The Tidewater Tales* (New York, 1987), 363; U.S. Census of Housing, 1980, Vol. 1: *Characteristics of Housing Units*, Chapter A: *General Housing Characteristics*. Newspaper circulation data are compiled by the Standard Rate and Data Service, *Newspaper Circulation Analysis*.

Los Angeles. Somewhat more inclusive is the idea of a Chesapeake "Crescent" arcing from Norfolk to Baltimore with Washington as its organizing midpoint.⁷⁰

Regional consciousness can also be traced through the choice of regional terminology in the names of businesses and organizations.⁷¹ City directories for the District of Columbia for 1912 through 1970 and combined city and suburban telephone directories for 1970 and 1986 have been examined for the use of three sets of regional terms as the initial word in business and organizational names. As Figure 2 indicates, "North" and its variations have been also-rans from the start. More interesting are a complex of "middle" terms. "East" may imply connection to the northeastern seaboard. It may also group with "Atlantic" and "middle" as a set of neutral names that avoid identification with either North or South. Taken in aggregate, these three regional identifiers passed southern terms in popularity soon after World War II and surged further ahead after 1970.⁷²

In a sense, Washington is constructing a new regional identity by selective borrowing from both models of its regional character. As "the most important city in the world," in a phrase common around town, Washington is defined as a key location for control functions in the national and international economies, and a location that is separate rather than subordinate to New York and the Northeast.⁷³ At the same time, it retains strong connections to the multiple Souths of tidewater towns, Carolina farms, and Texas military bases. The articulation of a contemporary consumption region around this emergent control center incorporates direct links to the traditional South but filters them through the needs of an information-era metropolis.

IT IS SOMETIMES ARGUED THAT CONTEMPORARY WASHINGTON has lost all regional character. To a self-defined outsider like President Jimmy Carter, it was an "island" with few bridges to the American mainland. Its communities of bureaucrats and lobbyists are thought to make the city "inside the Beltway" into an aberration that lacks the regional identification and loyalties found in more normal cities. "It's fine for spies and newspapermen," says one of the characters in Larry McMurtry's *Cadillac Jack*, "but it ain't everybody's cup of tea. Maybe you oughta move to Minnesota."⁷⁴

⁷⁰ Washington Board of Trade, "Marketing Concept for the Baltimore/Washington Common Market" (June 1976) and 1978 statement on business conditions, both in Board of Trade Papers, George Washington University; Diane Granat, "Is Washington-Baltimore the Next Silicon Valley?" *Washingtonian* (April 1987): S3-S22; Edwin T. Haefele, "Developing the Region's Economy," in Shidler, ed., *Greater Washington in 1980*, 149; Stephen Fuller, "Regional Economic Interdependence: The Washington/Baltimore Common Market," paper delivered at annual meeting of the Association of Collegiate Schools of Planning, November 1985; George Grier and Eunice Grier, *Greater Washington at Mid-Decade: Shifting Growth Trends and Their Implications for the Regional Future* (Washington, 1985).

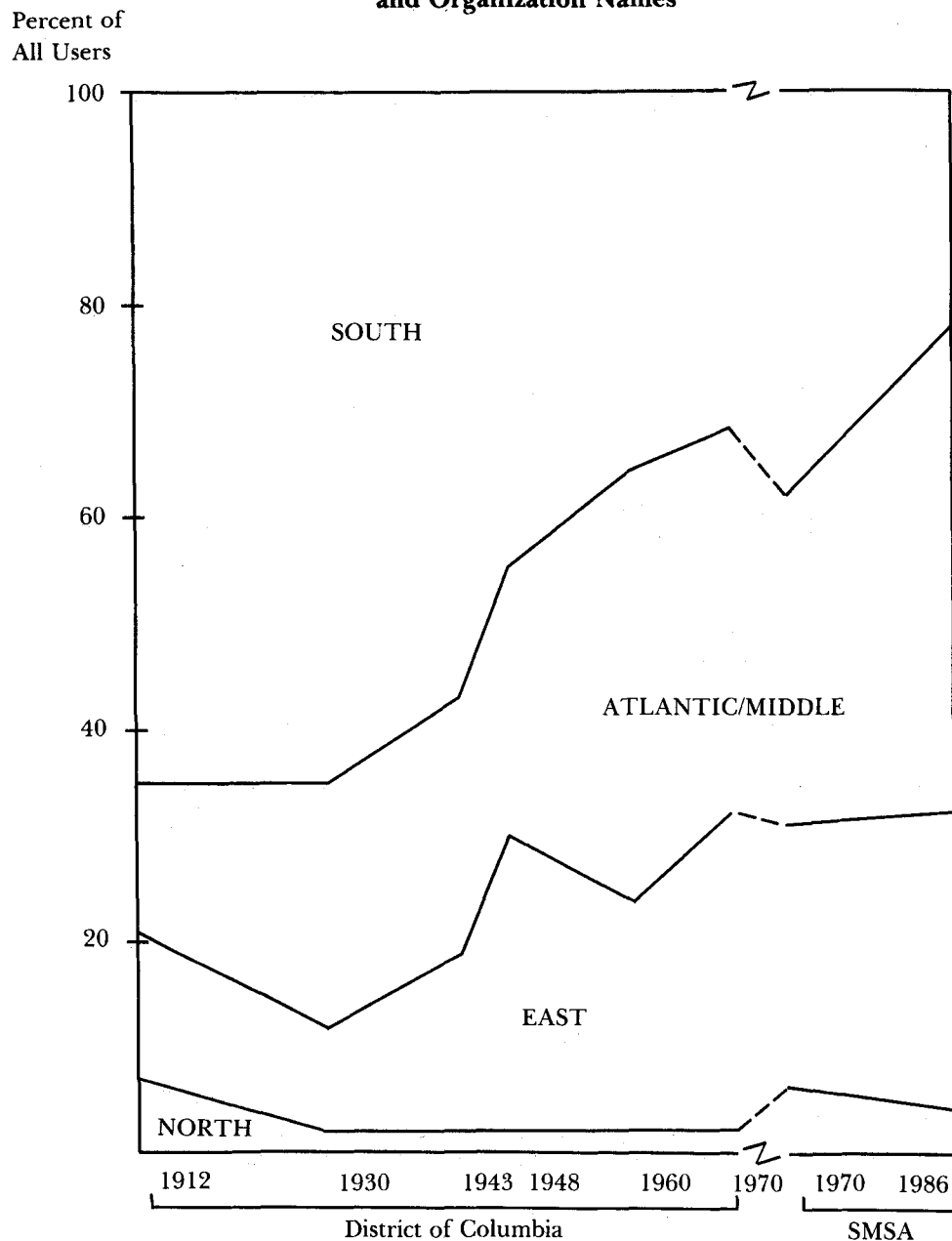
⁷¹ John Shelton Reed, "The Heart of Dixie: An Essay in Folk Geography," *Social Forces*, 54 (June 1976): 925-39; Wilbur Zelinsky, "North America's Vernacular Regions," *Annals of the Association of American Geographers*, 70 (1980): 1-16.

⁷² The tallies omit two types of regional usage that might confuse the data: first, "northwest," "northeast," "southeast," and "southwest" when they clearly referred to the location of Washington businesses within one of the city's standard street address quadrants; and second, regional terms that were obviously part of the name of national organizations located in Washington because of the presence of the federal government.

⁷³ William S. White, "Public and Personal: Home Town USA," *Harper's Magazine*, 220 (June 1960): 95; Johnson, "Capital of Success," 44; "Washington vs. New York," *Newsweek*, 112 (June 20, 1988): 16-24; William Regardie, interview, March 13, 1987; Joel Garreau, interview, March 25, 1987; Jack Limpert, interview, April 9, 1987.

⁷⁴ Larry McMurtry, *Cadillac Jack* (New York, 1982), 235; Joel Garreau, in *The Nine Nations of North America* (Boston, 1981), 104, described Washington as an aberration and also quoted Jimmy Carter on Washington as an island.

FIGURE 2
Regional Terms in Washington Business
and Organization Names



This analysis offers a different conclusion. Since the city's founding decades, two periods of particularly rapid expansion in permanent federal employment have triggered economic and demographic multipliers. Each era of change attracted new businesses to serve the federal government and its workers or to capitalize on

the expanded economic base and increased prominence of the city. Because of Washington's unique role as a national center, new residents have come from both an adjacent hinterland and a national pool of specialized workers. Between 1860 and 1930, the result was a relatively simple pattern in which an influx of northern businessmen and politicians failed to prevent a slow revitalization of southern characteristics. Changes since 1930 have involved a more complex balance of increased interaction with the North and maintenance of southern connections in ways facilitated by expanding roles within national systems.

Two other sets of American border cities offer possible comparisons for Washington. Cincinnati, Louisville, and St. Louis developed as nineteenth-century trading hinges between the growing Middle West and the central South. Although usually included in the industrial core, they also lie along the westward extension of the cultural boundary between North and South. Cincinnati's role as focus for white Appalachian migrants, for one example, may parallel that of Washington for black Carolinians. Analysis of evolving regional affiliation and connections along this historic steamboat frontier should further clarify the directions of regional change for the twentieth-century South.⁷⁵

Tulsa, Oklahoma City, Dallas, Fort Worth ("where the West begins"), Austin, and San Antonio are similarly located on the margin between South and West as both natural and cultural regions. Daniel Elazar's mapping of political culture puts all but Oklahoma City in the zone of overlap between Greater South and Greater West. Wilbur Zelinsky splits Texas and Oklahoma down the middle but also notes that each may be emerging as a smaller but separate region. Donald Meinig and others have seen the emergence of a "greater Texas" that mediates between South, West, and Middle West.⁷⁶ In ways parallel to Washington, other transactional centers such as Dallas or Austin may face tensions between the construction of new regional patterns and absorption into nonregionalized national and international systems.⁷⁷

The specific findings for Washington call into question not only the idea of a deracinated metropolis but also the common theoretical assumption of regional hegemony and unidirectional change in which an economically dominant and culturally aggressive core region absorbs peripheral areas and supplants their traditional cultures. Regional change in Washington has not operated in parallel in the public spheres of business and politics and the private realms of personal behavior and allegiance. Washington at the end of the twentieth century has experienced substantial and almost certainly permanent accommodation to the public values of the North and has been integrated into the communication networks of the northeastern core. At the same time, improvements in transportation and communication have enhanced its role as a metropolitan focus for southern culture. Its border location has worked as much to concentrate and

⁷⁵ Richard C. Wade, *The Urban Frontier: Pioneer Life in Early Pittsburgh, Cincinnati, Lexington, Louisville, and St. Louis* (Cambridge, Mass., 1959); William Philliber and Clyde McCoy, eds., *The Invisible Minority: Urban Appalachians* (Lexington, Ky., 1984); Zelinsky, *Cultural Geography*; Gastil, *Cultural Regions*; Bigelow, "Roots and Regions."

⁷⁶ Walter Prescott Webb, *The Great Plains* (New York, 1931); Howard W. Odum, *Southern Regions of the United States* (Chapel Hill, N.C., 1936); Carl Abbott, "The Metropolitan Region: Western Cities in the New Urban Era," in Gerald Nash and Richard Etulain, eds., *The Twentieth-Century West: Historical Interpretations* (Albuquerque, N.Mex., 1989), 71-98; Elazar, *American Federalism*; Zelinsky, *Cultural Geography*; Meinig, *Imperial Texas*.

⁷⁷ Reed, "Heart of Dixie," 938, noted that Dallas began to shift from a southern to a western self-identification with the economic boom in the Sunbelt during the early 1970s. Melville Branch, *Regional Planning: Introduction and Explanation* (New York, 1988), 69, cited data on the recognition of northern Texas and Oklahoma as a distinct consumer product market.

reconfirm southern connections as to facilitate northern incorporation. Such southern ties are not "relict cultural slopes," in the phrase of Wilbur Zelinsky, but rather an updating of connections as a southern destination point and staging ground for return contact.⁷⁸ The endurance of Washington's southern character despite strong cosmopolitan influences supports the larger argument for an enduring South that can modernize without northernizing. "New" has meant northern and megalopolitan, but it has also meant southern and Chesapeake.

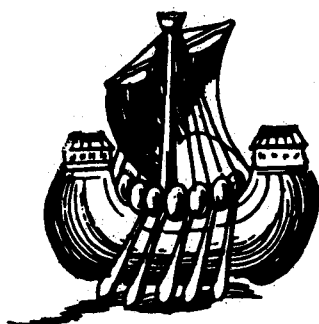
In theoretical terms, the study finds little support for the argument that contemporary regional culture is a form of resistance to the homogenizing and hegemonic forces of a globalized economy and its detached elites. This interpretation has been proposed by sociologists and regional planners such as Manuel Castells and Philip Cooke, particularly on the basis of national separatist movements in Europe. Pierre Clavel's study of "opposition planning" in Appalachia applies it to the United States. Clavel finds that regional identity itself becomes a tool of socio-political resistance against economic and political exploitation.⁷⁹ In Washington, however, the evidence suggests that regional orientation is largely a private choice not intended to convey a political message. Regional identity and connections provide many black Washingtonians with a separate sphere that supplements big-city life. For middle and upper-status residents of both races, they offer alternative arenas of consumption within the reach of an affluent metropolis. I find that the work of John S. Reed rather than the ideas of Castells and Clavel holds a greater similarity to the results of the Washington study. He has argued that the members of the new middle class in southern cities make self-conscious choices to incorporate traditional "southern" values and behaviors into their nontraditional lives. Whether raised inside or outside the region, such inhabitants of the new urban South construct behavior patterns that define distinctiveness within a larger modernizing society.⁸⁰ In the same way, the regional rootedness of blacks and the voluntary regionalism of whites are means of self-definition within a mobile society. Southernness in Washington survived the changes of the later nineteenth century largely intact and adapted to the further changes of the middle twentieth century. The city is substantially more northern now than in 1865, but it is also southern in new as well as old ways.

⁷⁸ Zelinsky, *Cultural Geography*, 117.

⁷⁹ Manuel Castells, "Space and Society: Managing the New Historical Relationships," in Michael Peter Smith, *Cities in Transformation: Class, Capital and the State* (Beverly Hills, Calif., 1984), 241-45; and Philip Cooke, *Theories of Planning and Spatial Development* (London, 1983), 161. Pierre Clavel, *Opposition Planning*, 68, asserted that "cultural differentiation also can reinforce local capacity. Resistance to external control, whether the resistance is organized by the local bourgeoisie or by some other group (such as the antigrowth coalition), requires a positive motivation and a set of symbols around which to develop a program."

⁸⁰ John Shelton Reed, *Southerners: The Social-Psychology of Sectionalism*; and *One South: An Ethnic Approach to Regional Culture* (Baton Rouge, La., 1982), 119-26.

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Moving Out and Settling In: Residential Mobility, Home Owning, and the Public Enframing of Citizenship, 1921–1950

RONALD TOBEY, CHARLES WETHERELL, and JAY BRIGHAM

HISTORIANS FROM FREDERICK JACKSON TURNER TO STEPHAN THERNSTROM have viewed residential mobility as a ubiquitous condition of American life, a logical response to expanding frontiers or shrinking local opportunities. In the 1960s, when historians studying the impact of urbanization, immigration, and industrialization on the American social order began to uncover the extraordinary extent of nineteenth-century residential mobility, most perceived it as a barometer of more fundamental change. Although historians gained considerable knowledge about migration as a collective process and as an individual experience, there is far more to the story.

Residential mobility fell from high levels in the nineteenth and early twentieth centuries in both rural and urban settings to a more modest and constant rate of approximately 20 percent a year after World War II. This dramatic change in a basic feature of American life warrants asking when, how, and why it occurred. The following study of Riverside, California, between 1921 and 1950 suggests that residential mobility did not decline gradually between the two world wars as historians such as Thernstrom have thought. Instead, nineteenth-century levels of mobility persisted throughout the interwar period and dropped sharply only after 1945. What put the brakes on residential mobility was neither the Great Depression nor the shock of World War II but rather the housing policies of President Franklin Roosevelt's New Deal state. More specifically, the Federal Housing Administration (FHA) and other New Deal agencies restructured the private housing market in a way that fundamentally altered the conditions under which Americans purchased and owned their homes.

The long-term mortgage was the instrument that finally restrained America's historic mobility. Before New Deal programs, home mortgages generally averaged less than ten years. The FHA doubled this term and in so doing stabilized an ailing home-loan industry and expanded opportunities for homeownership. It was also no accident. Roosevelt badly wanted to modernize the nation's housing stock, not simply in order to improve the quality of life for the American people but to engender a revitalized idea of citizenship that directly and specifically involved homeownership. Although the Lockean notion that citizens should have a property stake in society had persisted as a major theme in American political culture virtually undiluted since the eighteenth century, it was the sheer power of the New

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Deal state that made possible implementation of the idea. Between the two world wars, people in Riverside were continually moving out. Half of all homeowners left their dwellings within three years; three-quarters of all renters left within one year. Moreover, both in Riverside and nationally, the propensity for brief residential tenures differed marginally across socioeconomic lines. Only after the federal government changed the rules of home financing did Americans begin settling in after a century of unabated movement. This process took less than two decades and possibly less than ten years.

Nearly thirty years ago, Stephan Thernstrom revolted against the ahistorical methodology of functional sociology and launched a research agenda that a generation of social historians followed unrelentingly.¹ Although they ultimately failed to substantiate Thernstrom's thesis that the American social system was less open than cultural myth convinced Americans to believe, by 1982 social historians had completed the agenda Thernstrom set and rendered the study of historical social mobility virtually inert. Interest in residential mobility did not disappear completely with the demise of work on social mobility. Urban historians and geographers in the United States and England have continued to pursue the subject of residential mobility in ways that have refined the larger issues. Although they still focus on the general relationship between mobility and economic opportunity, historians are now asking specific questions about the availability of housing, demographic influences, the role of local government, and the weight of political and social position on individual decisions to move. Nonetheless, as Eric Monkkonen recently characterized the present state of affairs, "the precise relationship of high mobility to other social features is unclear, and it must therefore remain as a background feature of American society."²

Studies of both social and geographic mobility usually terminate in the first decades of the twentieth century for two reasons. First, the federal manuscript

¹ Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964). The literature of the new urban history is immense, but some of the most important works are Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, Conn., 1969); Peter R. Knights, *The Plain People of Boston, 1830-1860: A Study in City Growth* (New York, 1971); Howard Chudacoff, *Mobile Americans: Residential and Social Mobility in Omaha, 1880-1920* (New York, 1972); Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970* (Cambridge, Mass., 1973); Michael B. Katz, *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975); Theodore Hershberg, ed., *Philadelphia, Work, Space, Family, and Group Experience in the Nineteenth Century: Essays Toward an Interdisciplinary History of the City* (New York, 1981); and Michael B. Katz, Michael J. Doucet, and Mark J. Stern, *The Social Organization of Early Industrial Capitalism* (Cambridge, Mass., 1982). For a good summary of the literature, see Michael H. Ebner, "Urban History: Retrospect and Prospect," *Journal of American History*, 68 (1981): 69-84. On the legacy of *Poverty and Progress*, see John H. Ingham, et al., "Thernstrom's *Poverty and Progress*: A Retrospective after Twenty Years," *Social Science History*, 10 (1986): 1-44.

Urban historians have done most of the work on social mobility, but historians of rural America were the first to address systematically the question of residential mobility. See, for example, James C. Malin, "The Turnover of Farm Population in Kansas," *Kansas Historical Quarterly*, 4 (1935): 339-372; Merle Curti, *The Making of An American Community: A Case Study of Democracy in a Frontier Community* (Stanford, Calif., 1959); Allan G. Bogue, *From Prairie to Corn Belt: Farming on the Illinois and Iowa Prairies in the Nineteenth Century* (Chicago, 1963); Michael P. Cozen, *Frontier Farming in an Urban Shadow: The Influence of Madison's Proximity on Agricultural Development of Bloomington, Wisconsin* (Madison, Wis., 1971); Robert P. Swierenga, "Towards the 'New Rural History': A Review Essay," *Historical Methods Newsletter*, 6 (1973): 111-21; and Hal S. Baron, *Those Who Stayed Behind: Rural Society in Nineteenth-Century New England* (Cambridge, 1984). On the notion of a gradual decline in twentieth-century residential mobility, see Thernstrom, *Other Bostonians*, 10-15.

² Eric M. Monkkonen, *America Becomes Urban: The Development of U.S. Cities and Towns* (Berkeley, Calif., 1988), 197. See also Monkkonen, "Residential Mobility in England and the United States, 1850-1900," in *Themes in British and American History: A Comparative Approach* (London, 1985), 77-83.

census schedules that comprise the central class of evidence for studies of nineteenth-century mobility remain closed, forcing historians to depend on other, less reliable material.³ Second and more important, by the late 1970s, questions surrounding the impact of urbanization, immigration, and industrialization on the American social structure seemed moot, since each process was essentially completed by 1920. With the historical reality of residential mobility no longer in dispute, historians raised the interpretive stakes to the highest possible level. In much the same way Thernstrom maintained that high geographic mobility constituted an "American pattern" sustained by a "floating proletariat" constantly on the move, Michael Katz, Michael Doucet, and Mark Stern argued in the last major study of social mobility that "transiency" and inequality were the two "defining attributes of the social structure of industrializing capitalism."⁴ Thus, as historians abandoned the study of social mobility after 1920, sociologists assumed the task of studying it after World War II.⁵

An earlier generation of interwar social scientists had produced a classic literature on American towns and cities, yet their work was attacked by Thernstrom as ahistorical social anthropology. Robert Lynd and Helen Lynd's studies of Muncie, Indiana, *Middletown* (1929) and *Middletown in Transition* (1937), are the most famous of a group of studies of social stratification and integration that only glanced at residential mobility and examined historical social mobility not at all.⁶ The Lynds and their contemporaries portrayed communities as historically static, existing in a functionalist society without a past. Nor was residential mobility a problem of interest, because they understood it as merely a minor obstacle to the normal processes of social integration. Indeed, interwar sociologists viewed residential mobility as inherently pathological.⁷ If the social structure of a town held together, then, by definition, extreme residential mobility could not exist. All sociologists of the small city found resilient social structures with traditional social practices, such as class and racial segregation, that resisted pressures for change from growing populations, technological innovations, or shifting economic trends.

³ One need only consider the difficulties Thernstrom encountered trying to extend his analysis of Boston to 1970 to appreciate the severity of the problem of evidence. See Stanley L. Engerman, "Up or Out: Social and Geographic Mobility in the United States," *Journal of Interdisciplinary History*, 5 (1975): 469–89; Richard S. Alcorn and Peter R. Knights, "Most Uncommon Bostonians: A Critique of Stephan Thernstrom's *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970*," *Historical Methods*, 8 (1975): 98–114; and Stephan Thernstrom, "Rejoinder to Alcorn and Knights," *ibid.*, 115–20. Some historians have tried to uncover the extent and nature of out-migration using SOUNDex, a semi-automated, nominal record linkage procedure for some federal censuses. See Michael Katz and John Tiller, "Record-Linkage for Everyman: A Semi-Automatic Process," *Historical Methods Newsletter*, 5 (1972): 144–50; Charles Stephensen, "Determinants of American Migration: Methods and Models in Mobility Research," *Journal of American Studies*, 9 (1975): 189–97; Stephensen, "Tracing Those Who Left: Mobility Studies and the Soundex Indexes," *Journal of Urban History*, 1 (1974): 73–84; and Stephensen, "Migration and Mobility in Late Nineteenth- and Early Twentieth-Century America" (Ph.D. dissertation, University of Wisconsin, Madison, 1980).

⁴ Katz, Doucet, and Stern, *Social Organization of Early Industrial Capitalism*, 2.

⁵ There are, of course, exceptions. See Monkkenon, *America Becomes Urban*, 182–205.

⁶ Robert S. Lynd and Helen Merrell Lynd, *Middletown: A Study in American Culture* (New York, 1929); and *Middletown in Transition: A Study in Cultural Conflicts* (New York, 1937). Others include W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community* (New Haven, Conn., 1941); W. Lloyd Warner, *Democracy in Jonesville: A Study of Quality and Inequality* (New York, 1949); August Hollingshead, *Elmtown's Youth: The Impact of Social Change on Adolescents* (New York, 1949); James West, pseud. [Carl Withers], *Plainville, U.S.A.* (New York, 1945); and John Dollard, *Caste and Class in a Southern Town* (New Haven, 1937).

⁷ See, for example, Ernest W. Burgess, "The Growth of the City: An Introduction to a Research Project," in Robert E. Park, Ernest W. Burgess, and Roderick D. McKenzie, *The City* (1925; rpt. edn., Chicago, 1967), 59.

To this conservative resistance to change, the standardization of ideas and goods in a mass-production economy added the gloss of uniformity that reinforced group solidarity.⁸

Social scientists after 1945 relinquished their fear that residential mobility was inherently pathological. Residential change became so pervasive that it ceased to be unusual or threatening, and sociologists came to view personality disorder and antisocial behavior as stemming from more deeply rooted problems such as poverty. Peter Rossi, who has synthesized much of the research on residential mobility, stated simply that residential mobility is normal. For sociologists, the important question is no longer why people move but rather why people move where they do.⁹

ESTIMATES VARY, BUT THERE IS LITTLE DOUBT that extensive geographic mobility has always marked the historical American social order. It ranged from a low of 29 percent in Indianapolis between 1880 and 1890 to a high of 85 percent in St. Louis from 1840 to 1850, although the majority of places studied thus far experienced decennial rates of residential mobility in excess of 50 percent.¹⁰ To Thernstrom in 1973, existing work revealed a national pattern of high geographic mobility. More recently, Robert G. Barrows questioned Thernstrom's perception of uniformity, emphasizing instead the diversity of experiences and suggesting that historians needed to think more about contextual factors, such as the availability of housing, before advancing broad generalizations. In a study of Hamilton, Ontario, and Buffalo, New York, Katz, Doucet, and Stern confirmed the generally high (50 percent) rate of geographic mobility but also demonstrated that it did not vary significantly among workers in different economic sectors.¹¹ Yet, beyond suggesting that structural features of nineteenth-century populations such as age and property ownership constrained mobility, historians have been unable to discover any clear patterns in the differences in residential mobility from place to place and over time.

Sociological work on the period after 1920 reveals both continuities and discontinuities with the past. In their first study of Middletown, the Lynds discovered high residential mobility among some economic and social groups.¹² When the Lynds studied Middletown in the 1930s, they again found high rates of residential change. On this occasion, they paid attention to the difference between owners and renters and found relative stability among owners and rapid residential turnover among renters. Although the Lynds speculated that such different rates

⁸ Lynd and Lynd, *Middletown*, 490–501.

⁹ Peter H. Rossi and Anne H. Shlay, "Residential Mobility and Public Policy Issues: 'Why Families Move' Revisited," *Journal of Social Issues*, 38 (1982): 21–34; see also Peter H. Rossi, *Why Families Move: A Study in the Social Psychology of Urban Residential Mobility* (Glencoe, Ill., 1955).

¹⁰ Decennial mobility averaged 54.5 percent ($s = 13.2$), which is based on data drawn from Thernstrom, *Other Bostonians*, table 9.1, 222–23; Robert G. Barrows, "Hurrying Hoosiers and the American 'Pattern,' Geographic Mobility in Indianapolis and Urban North America," *Social Science History*, 5 (1981): table 2, 203–04; and Katz, Doucet, and Stern, *Social Organization of Early Industrial Capitalism*, 107, 109, 122.

¹¹ Thernstrom, *Other Bostonians*, 220–61; Barrows, "Hurrying Hoosiers," 201–05; and Katz, Doucet, and Stern, *Social Organization of Early Industrial Capitalism*, 103–30.

¹² Of families in what they termed the business class, 45 percent had moved once between 1920 and 1924, but only 10 percent had moved more than once. Of families in the working class, however, 40 percent had moved once between 1920 and 1924, while 33 percent moved more than once; Lynd and Lynd, *Middletown*, 520.

of residential mobility within "two parts of the same community suggest deep-lying implications," they did not pursue the matter.¹³

The U.S. Bureau of the Census itself did not become interested in geographic mobility until the late 1930s and only investigated it systematically after 1947.¹⁴ In that year, the bureau estimated that 36.4 percent of the civilian population resided in the same house they had occupied in 1940, another 41.8 percent had moved to a different dwelling in the same county, and 21.8 percent had moved out of the county altogether. From 1948 on, the bureau annually collected information on residential mobility for the preceding year. Approximately 20 percent of all Americans have moved every year since 1948, a figure that has held steady despite population growth, two wars, and economic cycles of prosperity and recession. Not until 1960, however, did the Census Bureau gather data that would again allow it to analyze mobility for periods longer than a single year.¹⁵

The lack of strong data on residential change and therefore of historical studies on residential mobility in the interwar decades constitutes a serious gap in historians' understanding of changes in residential mobility. Although we have estimates of county, state, and regional migration of varying quality from 1870 to 1950, we cannot establish a sound historical trend in residential mobility through both the nineteenth and twentieth centuries.¹⁶ Nineteenth-century mobility appears high, and post-World War II mobility seems high also, but less high. We have the early and the late points but not the middle points on a possible trend line. Consequently, we can neither determine when mobility decreased nor distinguish between the effects of shifting structural features of American society and the particular circumstances of different historical periods.

The variety of methods historians, sociologists, and geographers have employed also hampers efforts to establish a single pattern of residential mobility. Historians have estimated residential mobility over five and ten-year periods largely on the basis of the disappearance of names from various sources. Sociologists and demographers have estimated mobility over one and five-year periods using more reliable survey data. The cross-sectional approach followed by both groups limits their ability, however, to generalize about mobility over longer periods of time, at least before 1948. It may have been the case that, until 1920, the majority of residents left a city within ten years, but it may also have been the case that they moved several times within the city before leaving.

Rates of historical mobility obtained from cross-sectional analyses, moreover, do not measure residential tenure, that is, how long people lived in a particular

¹³ Only 3.8 percent of homeowners had lived for less than a year in their dwellings, as compared with 42.2 percent of sampled renters. Lynd and Lynd, *Middletown in Transition*, 187–88, and table 21, 553. The Lynds actually used the results of the Bureau of Foreign and Domestic Commerce's Real Property Inventory of 1934 for Muncie, Indiana, which included information on length of residency but which unfortunately was not included in the published reports. Bureau of Foreign and Domestic Commerce, *Instructions to Real Property Inventory Enumerators, General Instructions* (Washington, 1933), 16.

¹⁴ Margo J. Anderson, *The American Census: A Social History* (New Haven, Conn., 1988), 159–212.

¹⁵ Bureau of the Census, "Internal Migration in the United States: April, 1940, to April, 1947," *Current Population Reports: Population Characteristics*, Series P-20, No. 14 (April 15, 1948), table 1, 13; Bureau of the Census, "Mobility of the Population of the United States, March 1970 to March 1971," *Current Population Reports: Population Characteristics*, Series P-20, No. 235 (April 1972), table 1, 8–9.

¹⁶ See especially Henry S. Shyrock, Jr., *Population Mobility within the United States* (Chicago, 1964), 6–90; Donald J. Bogue, *Components of Population Change, 1940–1950: Estimates of Net Migration and Natural Increase for each Standard Metropolitan Area* (Oxford, Ohio, 1957); Donald J. Bogue, *The Population of the United States* (Glencoe, Ill., 1959), 275–418; and Simon Kuznets and Dorothy Thomas, eds., *Population Redistribution and Economic Growth in the United States, 1870–1950*, 3 vols. (Philadelphia, 1957–64).

dwelling. A resident who appears in only one census schedule could conceivably have lived in the dwelling since the day after the previous census was taken, thereby having a residential tenure of ten years. Just as census schedules may mask higher rates of residential mobility than historians report, they may also mask lower ones. Richard Jensen, for instance, argued that the general picture of high mobility exaggerates reality and historians will ultimately be forced to lower their estimates because they have "casually assumed that everyone who is not traced in the same place must have migrated."¹⁷ In general, the limitations of the records most historians use to document residential mobility suggest that the real issue is not how many people stayed in the same place or the same house for one year or for ten years but rather the actual duration of residence.

Historical and contemporary estimates of residential mobility often refer to different groups within the larger population and reflect different ideas about what constitutes mobility. Most estimates of historical geographic mobility pertain to either adult males or heads of households, those groups most easily traced across manuscript census schedules, city directories, and tax rolls. The Census Bureau's estimates of post-World War II mobility, however, pertain to the entire population and obscure age-specific patterns.¹⁸ More important is the issue of what constitutes residential mobility. According to Donald Bogue, population studies distinguish between local moves within a community and migration to a different community as a matter of course.¹⁹ The view that dominates historical and contemporary studies, however, equates mobility with migration out of a specific city or county. In 1947, the Census Bureau defined migrants as those who did not reside in the same county they had in 1940. Those who lived in a different house but in the same county were not designated migrants. In short, the Census Bureau's almost exclusive focus on intercounty movement drastically limits our ability to evaluate interwar patterns of residential mobility.

Finally, most historical sources and contemporary Census Bureau surveys fail to record the information historians and sociologists are coming to believe is most important: whether people rented or owned their homes. The bureau does not report tenure status in its annual analyses of residential mobility, and, with only a few exceptions, historical studies fail to consider it at all. Katz, Doucet, and Stern found that property holding significantly constrained movement. Howard Chudacoff found the highest incidence of residential mobility in Omaha, Nebraska, to be in an area that contained the greatest number of apartments and boarding

¹⁷ Richard Jensen, "Found: Fifty Million Missing Americans," paper presented at the Annual Meeting of the Social Science History Association (November 8, 1980), 11. Richard G. Bremer, *Agricultural Change in an Urban Age: The Loup Country of Nebraska, 1910-1970* (Lincoln, Neb., 1951), found that adjusting mobility for mortality changed the picture very little.

¹⁸ Young adults have tended to be more mobile than either the very young or the very old in both the nineteenth and twentieth centuries. See, for example, Katz, Doucet, and Stern, *Social Organization of Early Industrial Capitalism*, 104-08; Bureau of the Census, *Current Population Reports*, Series P-20, "Geographical Mobility: March 1975 to March 1980" (1981), table 6, 18; *idem*, "Mobility of the Population of the United States, March 1970 to March 1975," *Current Population Reports: Population Characteristics*, Series P-20, No. 285 (October 1975), table 28, 61; *idem*, *1970 Census of Population, General Social and Economic Characteristics, United States Summary* (June 1972), table 148, 1-481; *idem*, *Census of the Population: 1960, Vol. 1: Characteristics of the Population, Number of Inhabitants, General Population Characteristics, General Social and Economic Characteristics, and Detailed Characteristics, Part 1, United States Summary* (1964), 1-367; *idem*, "Mobility of the Population of the United States March 1957 to 1958," *Current Population Reports: Population Characteristics*, Series P-20, No. 88 (October 1958), table 4, 10; and D. Bogue, *Population of the United States*, 379-81.

¹⁹ D. Bogue, *Population of the United States*, 375.

houses.²⁰ The Lynds found that during the Great Depression half of all renters had moved within the previous year. These findings point to a fundamental distinction that needs more thorough exploration, especially for a nation in which most people rented their homes. The determination of residential tenure over more than a one or a five-year interval for both homeowners and renters first of all requires appropriate historical records. Riverside, California, has such records.

OUR ANALYSIS OF RESIDENTIAL MOBILITY is based on 8,774 instances of individual tenure in a sample of 1,573 electrified housing units drawn from the Utility Department billing records of the City of Riverside between 1921 and 1950.²¹ Utility billing records represent a new and extraordinarily revealing source for studying residential tenure. Not only do they provide information on how long occupants received bills, and hence how long they occupied dwellings, but also whether they owned or rented dwellings. Because the billing records are monthly, they allow us to analyze residential tenure with heretofore unattainable precision. Furthermore, the information on ownership is unequivocal. If an individual owned the dwelling that he or she occupied, the word "owner" was printed by the name; if a renter, his or her name was listed along with the dwelling's owner or the individual who guaranteed payment for electrical consumption charges.

Although unusually detailed, the billing records are biased. For instance, the extent of homeownership among sample individuals exceeds the national average (although it does mirror the incidence of homeownership in Riverside).²² However, this particular bias renders our estimates of residential mobility in Riverside conservative, because homeowners tended to persist for longer periods than renters. Also, there are residents whose tenure extends beyond 1950, when the records terminate. As a result, the population likely to be shown residing in

²⁰ Chudacoff, *Mobile Americans*, 37–38, 58–60.

²¹ The sample of 1,573 electrified dwellings represents an average of 9 percent of all electrified dwellings in Riverside between 1921 and 1950. Dwellings themselves were selected from an 80 percent random sample of all electrical utility billing company meter routes established during those years. The sample reflects both the increase in housing stock in Riverside as well its geographical dispersion. City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside; hereafter cited as Riverside Utility Billing Records.

²² A comparison of the proportions of owner-occupied dwellings in the nation and in Riverside is as follows:

	1920 (%)	1930 (%)	1940 (%)	1950 (%)	1960 (%)
Nation	40.6	36.0	41.1	53.4	61.0
Riverside					
Census			50.5	55.4	
Sample		61.0	55.4	72.2	

The sample proportions are not significantly different from those reported by the census for Riverside in either 1940 or 1950 (1940: $X^2 = 1.6$, $p < .05$; 1950: $X^2 = 3.2$, $p < .05$). Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1970*, 2 vols. (Washington, 1975), Series N, 243, 2: 646; *idem*, *Sixteenth Census of the United States: 1940, Housing*, Vol. 1: *Data for Small Areas*, Part 1 (Washington, 1943), 141; *idem*, *Census of Housing: 1950*, Vol. 1: *General Characteristics*, Part 2 (Washington, 1953), 5–22; and Riverside Utility Billing Records, 1921–1950.

TABLE 1
Composite Rates of Residential Mobility, 1850–1970

<i>Interval (Years)</i>	<i>National 1850–1920 (%)</i>	<i>Omaha 1880–1920 (%)</i>	<i>Riverside 1921–1945 (%)</i>	<i>National 1948–1970 (%)</i>
1	—	—	49	20
5	—	77	82	46
10	55	91	88	—

SOURCES: National ten-year estimates, 1850–1920, see note 10; Omaha estimates, Howard Chudacoff, *Mobile Americans: Residential and Social Mobility in Omaha, 1880–1920* (New York, 1972), 36, 41 (Chudacoff's ten-year estimates are actually for eleven years); Riverside estimates, City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside; national 1948–1970; one-year estimates, Bureau of the Census, "Mobility of the Population of the United States, March 1970 to March 1971," *Current Population Reports: Population Characteristics*, Series P-20, No. 235 (April 1972), table 1, 8–9; five-year estimates, *idem*, *Current Population Reports*, Series P-20, "Geographical Mobility: March, 1975 to March, 1980" (1981), table 6, 18; *idem*, "Mobility of the Population of the United States, March 1970 to March 1975," *Current Population Reports: Population Characteristics*, Series P-20, No. 285 (October 1975), table 28, 61; *idem*, *1970 Census of Population, General Social and Economic Characteristics, United States Summary* (June 1972), table 148, 1–481; *idem*, *Census of the Population: 1960, Vol. 1: Characteristics of the Population, Number of Inhabitants, General Population Characteristics, General Social and Economic Characteristics, and Detailed Characteristics*, Part 1, *United States Summary* (1964), 1–367.

dwelling for longer periods decreases as the start of their tenure approaches 1950.²³

We can neutralize any occupancy bias if we adopt the cross-sectional approach most historians of nineteenth-century social mobility employ. By selecting those individuals in residence in a given year, we can determine whether they still lived in the same dwelling one, five, or ten years later. Table 1 displays a composite view of mobility rates drawn from historical and Census Bureau studies as well as the Riverside data. Together, these estimates indicate that a profound shift in residential mobility occurred after World War II. The high rates of mobility that historians have found around the nation in the nineteenth and early twentieth centuries characterized Riverside during the interwar period. After 1948, the national

²³ A related bias involves the first residents in sample dwellings. Since our information is drawn from electrical billing records, we cannot be absolutely certain that the first resident in a dwelling actually began residency when the dwelling was electrified; the dwelling may simply have been retrofitted with electricity. Because electrification was proceeding at such a rapid pace and most sample dwellings were, in fact, new structures, the effect of this particular bias is minimal. Of the 8,774 instances of residency, 6,158 cases of residency (70.2 percent) are completely captured in the records and possess no bias whatsoever. Another 1,052 (12.0 percent) were the first in the dwelling but concluded before 1950, and 1,069 (12.0 percent) extended beyond 1950. Only 495 cases (5.6 percent) crossed both the initial and terminal dates of the available records. (The Utility Department computerized its billing system in the late 1950s but subsequently destroyed all records from that decade.) If we assume that residency began with electrification, which we do, we face the risk of underestimating the tenure of the 1,547 persons (or 17.6 percent of sample cases) who appeared first in a sample dwelling. Billing records for 1,564 individuals (or 17.8 percent of sample cases) terminate in 1950, thus we cannot determine the exact lengths of their tenure. All we know is that they extended beyond 1950. Nonetheless, we have far fewer instances of long residential tenure than we do of short ones; 7,210 instances of individual residency (or 82.2 percent of the sample, including those individuals who were the first residents in dwellings) are completely captured by the records, which makes the sample far more complete than it first appears. The discrepancy between the size of the housing sample (1,573) and the number of cases of residency terminating in 1950 (1,654) can be accounted for by the tenure of nonresident landlords, who have been excluded from consideration.

TABLE 2
Mobility among Homeowners and Renters in Riverside, California, at
Five-Year Intervals, 1925–1950

	<i>Owners and Renters</i>		<i>Owners</i>		<i>Renters</i>	
	<i>Total N</i>	<i>% Mobile</i>	<i>Total N</i>	<i>% Mobile</i>	<i>Total N</i>	<i>% Mobile</i>
1925–30	519	59.2	355	44.5	164	90.9
1930–35	835	59.8	509	40.9	326	89.3
1935–40	1,019	62.0	507	34.1	512	89.6
1940–45	1,230	62.0	681	41.6	549	87.4
1945–50	1,387	52.6	952	41.8	435	76.3

SOURCE: City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside.

incidence of annual mobility dropped by more than one half; the five-year rate by more than 40 percent.

The Riverside data permit us to examine in detail the composition of historically high mobility and to glimpse signs of its attenuation after 1945. The first clue to understanding residential mobility in Riverside involves the distinction, first suggested by the Lynds and since demonstrated convincingly by Katz, Doucet, and Stern, between owners and renters. The cross-sectional estimates given in Table 2 reveal not only that mobility was high for both owners and renters throughout the interwar period but also that any analysis of residential mobility must consider the difference in tenure or title. Among homeowners, roughly 40 percent were not present in the same dwelling after 5 years. Among renters, mobility was astonishingly high. Between 1925 and 1945, more than 85 percent of all renters did not maintain the same residence for more than 5 years. Indeed, the length of residential tenure for both groups was short. For all 7,210 individuals completely captured in the utility billing records, it averaged less than 2 years. For the 2,178 homeowners, tenure averaged 3.7 years, and for the 5,032 renters, 1.1 years. The median length of tenure for owners was 1.9 years and for renters a mere six months.²⁴ Between the two world wars, people in Riverside, California, did not stay long in the same dwelling.

Structural changes in the economy do not appear to have affected mobility. Table 3 displays the cumulative proportions of Riverside homeowners and renters who remained in the same dwelling after selected years between 1921 and the end of 1950. The six periods correspond to historical changes in the nation's housing stock. Between 1921 and 1925, the nation enjoyed a housing boom, while from 1926 through 1930, the housing market collapsed. Between 1931 and 1936, the

²⁴ The relevant statistics on tenure (in years) for those householders whose residency did not extend beyond 1950 are as follows:

	<i>All</i>	<i>Owners</i>	<i>Renters</i>
Mean	1.9	3.7	1.1
s	3.2	4.6	1.7
se	0.04	0.1	0.02
Median	0.8	1.9	0.6
N	7,210	2,178	5,032

TABLE 3
Cumulative Proportions of Homeowners and Renters Persisting in Riverside, California, in Selected Years, by Cohort, 1921–1945

<i>Years</i>	<i>1921–25</i> (%)	<i>1926–30</i> (%)	<i>1931–36</i> (%)	<i>1937–41</i> (%)	<i>1942–45</i> (%)	<i>1946–50</i> (%)	<i>Average</i> (%)
<i>Homeowners</i>							
0	83.0	76.0	83.5	82.4	77.7	81.0	80.6
1	66.9	60.7	67.2	67.7	61.8	70.2	65.8
2	55.0	54.5	58.5	57.6	51.8	63.1	56.8
3	49.3	47.1	53.1	50.1	44.9	58.7	50.5
4	44.0	41.8	47.7	43.3	40.7	50.2	44.6
5	40.8	38.6	42.6	38.2	—	—	40.1
10	28.8	22.9	26.8	26.2	—	—	26.2
20	16.4	11.7	—	—	—	—	14.1
N	507	433	369	443	633	1,007	3,392
<i>Renters</i>							
0	30.9	26.1	35.3	30.0	36.6	51.3	35.0
1	16.7	11.3	17.4	15.0	19.1	31.2	18.5
2	12.3	7.4	10.4	9.5	14.0	21.9	12.6
3	8.5	5.8	6.7	7.2	10.7	16.1	9.2
4	6.9	4.6	5.0	5.2	7.9	12.8	7.1
5	4.7	3.3	3.7	—	—	—	3.9
10	3.2	0.8	1.3	—	—	—	1.8
20	1.0	0.1	—	—	—	—	0.6
N	317	782	1,367	1,312	924	680	5,382

SOURCE: City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside.

depression most strongly affected housing because banking crises decreased the availability of mortgage loans. In 1934, the federal government entered the housing market through the Federal Housing Administration, but it was not until 1937 that housing starts again reached pre-depression levels. The last two periods capture both the World War II and the postwar housing booms.

As with the cross-sectional view (Table 2), low levels of persistence predominated among both homeowners and renters. Approximately 20 percent of all homeowners moved within a year, half within three to four years. Similarly, 60 to 70 percent of renters moved within a year and 90 percent within three years. Despite the consistent patterns of persistence, we can see two changes. First and most dramatic is the increase in length of tenure among renters during and after World War II. Renter mobility within a year decreased from roughly two-thirds before 1940 to approximately one-half in the immediate postwar period. Second is the less dramatic but no less evident slowing in homeowner mobility after the first year of residence.

In order to assess these changes, it seems helpful to summarize. We are arguing that residential tenure in Riverside paralleled the general nineteenth-century experience until 1945. Although renters moved more often than homeowners, high residential mobility characterized both groups. Taken as a group, the pronounced differences in residential mobility between renters and owners before the war, the lower overall mobility after 1948, and the substantial in-

crease in homeownership nationally after the war suggest that increased homeownership was responsible for ending America's historical pattern of high residential mobility.²⁵

Such an explanation fails, however, to account for the increase in residential tenure among both renters and homeowners in the immediate postwar period. A temporary housing shortage and the extension of wartime rent controls to 1949 better explain the higher rates of persistence in immediate postwar Riverside than the general increase in home owning. Following the war, Riverside experienced a severe housing shortage estimated at 3,500 by the head of the local chamber of commerce—no small number in a city with approximately 12,131 housing units in 1945.²⁶ Communities in California and the nation shared Riverside's dilemma. Federal cost guidelines on housing for the military and for veterans, limited supplies of building materials, and postwar inflation all restricted home construction. In addition, national wartime controls that prohibited nonmilitary housing construction through 1946 exacerbated the situation.²⁷

Rent controls placed another constraint on residential mobility. In 1942, the Federal Office of Price Administration imposed controls on rental housing, hotels, and rooming houses, first in 302 national "defense areas," including Riverside, then in virtually every city in the nation.²⁸ Wartime controls initially lapsed July 1, 1946, and landlords raised rents, often by 50 to 100 percent. Renters across the nation stormed municipal legislatures, and Congress soon reauthorized and reimposed controls. Subsequent rent control relaxation came slowly, amid a protracted, five-year political brawl between landlord and tenant organizations, with landlords threatening to withdraw rental property from the market, to evict tenants en masse, or to cease signing rental contracts. In 1949, Congress authorized states, which in turn allowed municipalities, to decide whether to impose ceilings on rents. In Riverside, landlords swiftly won the battle, and on November 16, 1949, rent control ended in the city.²⁹

²⁵ The proportion of owner-occupied housing units increased from 41.1 percent in 1940 to 53.4 percent in 1950, and to 61 percent in 1960. *Historical Statistics*, Series N, 243, 2: 646.

²⁶ R. B. Hampson (President, Riverside Chamber of Commerce), "Hampson Appeals for Help in Housing," *Riverside Daily Press* (January 1, 1946), 5; Bureau of the Census, *Census of Housing, 1950: Vol. 1, General Characteristics*, Part 2 (Washington, 1953), table 20, 5–40.

²⁷ "Truman Cuts Major Housing Restrictions," *Riverside Daily Press* (December 16, 1946), 1; see also "Creedon Junks Former Housing Priority System," *ibid.* (December 24, 1946), 2. In 1947, the California legislature estimated that its immediate and future needs exceeded 700,000 units. Editorial, "Housing Crisis Will Be Worse before It Improves," *Riverside Daily Press*, February 21, 1946; "Housing Shortage in State Remains Acute," April 11, 1947; and "State's Critical Need of Housing Shown to C of C," June 17, 1947. The federal government estimated in 1946 that the nation needed 2,515,000 new houses and apartments in order to meet demand from the families of both veterans and nonveterans, a figure that exceeded the total number of housing units constructed from 1940 through 1945. "End of National Housing Emergency Not Yet Foreseen," *ibid.*, December 26, 1946; and *Historical Statistics*, Series N, 156, 2: 639. No comprehensive history of the postwar housing crisis exists, but see Richard O. Davies, *Housing Reform during the Truman Administration* (Columbia, Mo., 1966), 40–58; Paul F. Wendt, *Housing Policy—The Search for Solutions: A Comparison of the United Kingdom, Sweden, West Germany, and the United States since World War II* (Berkeley, Calif., 1962), 163–71; and Mary K. Nenno, "Housing the Decade of the 1940s—The War and Post-War Periods Leave Their Marks," in Gertrude Sipperly Fish, ed., *The Story of Housing* (New York, 1979), 242–67.

²⁸ "OPA Freezes Rental Prices for Housing," *Riverside Daily Press*, April 29, 1942; "Operation of Rent Control Explained," *ibid.*, May 9, 1942; "Roomers' Rents Now under Control," July 11, 1942; and "Control of Rents Extended," October 12, 1942.

²⁹ "Rent Control Ended Here, Woods Says," *Riverside Daily Press*, November 16, 1949. On the history of rent control, see Neil H. Lebowitz, "Above Party, Class, or Creed: Rent Control in the United States, 1940–1947," *Journal of Urban History*, 7 (1981): 439–70; Monica R. Lett, *Rent Control: Concepts, Realities, Mechanisms* (New Brunswick, N.J., 1976), 1–26; Ronald Lawson, ed., *The Tenant Movement in New York City, 1904–1984* (New Brunswick, N.J., 1986); and Allan David Heskin, *Tenants and the American Dream:*

Housing shortages and the postwar rent controls certainly lowered residential mobility. A 4.4 percent nationwide vacancy rate in 1950, the lowest in the postwar era, meant that few new dwellings were available for people to buy or rent.³⁰ Rent controls also decreased incentives to move. While wages and prices inflated as federal controls were selectively lifted, rents remained stable, and renters saw their relative standard of living increase simply because the proportion of their budgets consumed by housing costs declined. At the same time, if they chose, renters could accelerate accumulation of a down payment on a house by staying in a rent-controlled dwelling. Collectively, both the housing shortage and rent controls provided incentives to stay in a rent-controlled dwelling as long as possible, to ride out the housing crisis, and to put one's household in a stronger position to purchase a home when the crisis ended.

Once these conditions disappeared, as they did by 1949, residential mobility among renters might well have returned to pre-war levels. Yet residential mobility declined after World War II both in Riverside and across the nation. In 1945, 38 percent of Riverside heads of household had lived in the same dwelling five years earlier; in 1950, that proportion was 47.4 percent. In 1960, 63 percent of Riverside's population over five years of age had not moved since 1955.³¹ Nationally, the picture was much the same. As Table 4 reveals, between 1940 and 1960, national five-year mobility rates dropped from 59 to 48.7 percent; a 21.1 percent decline. At the same time, the proportion of owner-occupied dwellings increased nationally from 41.1 to 61 percent; an increase of 48.7 percent. Although these figures are not strictly comparable, they strongly suggest that a fundamental change in mobility occurred after 1950 and that the change was directly related to home owning.

Unfortunately, we cannot establish an unequivocal empirical relationship between home owning and persistence after 1950. The individual-level Riverside data end precisely when we suspect the greatest change occurred, and contemporary census data do not allow us to analyze tenure and persistence with much precision. Nonetheless, national statistics permit us to see the difference between the pre-war and postwar eras in the population's propensity to move. The picture confirms what the individual-level Riverside data suggest. For instance, Figure 1 displays the proportions of heads of households remaining at the same address for twenty years in Omaha between 1880 and 1920, in Riverside between 1921 and 1945, and for the entire population of the United States between 1940 and 1960, then again between 1950 and 1970. The levels of persistence in interwar Riverside and late nineteenth and early twentieth-century Omaha differ so markedly from those in postwar America that we are driven to the conclusion that a basic structural change in the mechanisms affecting mobility occurred in the middle of the twentieth century. Of this there seems little doubt.

We can explore in a limited fashion the structural characteristics of our sample population by using information contained in the twenty city directories issued in

Ideology and the Tenant Movement (New York, 1983). Although an advocacy pamphlet, Edith Bergen Drellich and Andree Emery, *Rent Control in War and Peace* (New York, 1939), contains useful information on World War I controls; similarly useful is Edith Elmer Wood, *Recent Trends in American Housing* (New York, 1931), 95–104.

³⁰ "Housing Units Vacancy Rates, by Region: 1940 to 1970," *Historical Statistics*, Series N, 247, 2: 646.

³¹ Bureau of the Census, *Census of Housing: 1970, Subject Reports, Final Report HC(7)-5, Mover Households* (Washington, 1973), 1.

TABLE 4
Five-Year Mobility and Persistence Rates, United States, 1940–1980

Period	Total Population (000)	Persisters		Movers	
		(N)	(%)	(N)	(%)
1975–80	198,403	107,257	54.1	91,146	45.9
1970–75	179,489	99,651	55.5	79,838	44.5
1965–70	173,657	98,564	56.8	75,093	43.2
1955–60	154,527	79,331	51.3	75,196	48.7
1940–47	122,034	52,136	42.7	69,898	57.3
1935–40	344,543	141,208	41.0	203,335	59.0

NOTES: The 1935–1940 figures reflect the number of heads of household in the 1940 Public Use Sample.

SOURCES: Bureau of the Census, "Geographical Mobility: March, 1975 to March, 1980," *Current Population Reports*, Series P-20 (1981), table 6, 18; *idem*, "Mobility of the Population of the United States, March 1970 to March 1975," *Current Population Reports: Population Characteristics*, Series P-20, No. 285 (October 1975), table 28, 61; *idem*, *1970 Census of Population, General Social and Economic Characteristics, United States Summary* (June 1972), table 148, 1–481; *idem*, *Census of the Population: 1960*, Vol. 1: *Characteristics of the Population, Number of Inhabitants, General Population Characteristics, General Social and Economic Characteristics, and Detailed Characteristics*, Part 1, *United States Summary* (1964), 1–367; *idem*, "Internal Migration in the United States: April, 1940 to April, 1947," *Current Population Reports: Population Characteristics*, Series P-20, No. 14 (April 1948), table 1, 13. The data for 1935–1940 were drawn from the *Census of Population, 1940 [United States]: Public Use Microdata Sample*, originally collected by the U.S. Department of Commerce, Bureau of the Census, and made available by the Inter-university Consortium for Political and Social Research. Neither the collector of the original data nor the Consortium bear any responsibility for the analyses or interpretations presented here.

Riverside between 1921 and 1950.³² Of the 8,774 individuals in the utility billing record sample, we were able to determine the occupations of 4,189, or 47.7 percent.³³ Although Riverside's city directories tended to over-represent the less mobile home owning members of the population, we found no clear indication that home owning was related to income except at the high end of the occupational scale.³⁴ As Table 5 reveals, only a majority of upper white-collar workers owned their homes; most householders in all other occupational groups rented. High

³² We consulted the *Riverside City Directory* for the years 1921, 1923, 1925, 1926, 1927, 1929, 1930, 1931–32, 1933, 1934, 1936, 1937, 1939, 1941, 1942, 1943, 1945, 1947, 1949, 1951, all published in Los Angeles.

³³ Another 3,049 (34.8 percent) were not listed in any city directory, either because a directory was not issued in the period of residence recorded in the billing records or because the directory failed to record the known resident. We excluded another 1,536 individuals who were listed in one or more directories but whose listings had no occupational information.

³⁴ As we might expect, those listed in one or more directories had significantly longer residential tenures than those who were not listed, 3.8 as compared with 1.6 years ($t = -23.67$, $df = 8,772$, $p > .01$). Similarly, the directory sample contained proportionately more homeowners than the billing record sample, 57.3 as compared with 38.7 percent. Because homeowners generally tended to persist longer than renters, however, they were more at risk to be recorded. Occupation groupings follow Thernstrom, *Other Bostonians*, 289–302, which modifies Census Bureau classifications established by Alba Edwards; Alba M. Edwards, "A Social-Economic Grouping of the Gainful Workers of the United States," *Journal of the American Statistical Association*, 28 (1933): 377–87. See also Katz, Doucet, and Stern, *Social Organization of Early Industrial Capitalism*, 39–63, for a class model.

TABLE 5
Proportions of Homeowners and Renters, by Occupational Group,
Riverside, 1921-1950

<i>Occupational Group</i>	<i>All</i>		<i>Homeowners</i>		<i>Renters</i>	
	<i>N</i>	<i>(%)</i>	<i>N</i>	<i>Group (%)</i>	<i>N</i>	<i>Group (%)</i>
High White Collar	1,537	36.7	899	58.5	638	41.5
Low White Collar	815	19.5	337	41.3	478	58.7
Skilled	767	18.3	363	47.3	404	52.7
Semi-Skilled	691	16.5	203	29.4	488	70.6
Unskilled	379	9.0	142	37.5	237	62.5
Totals	4,189	100.0	1,944		2,245	

SOURCES: City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside, and sources listed in note 32.

white-collar workers in Riverside persisted longer than those in other occupational groups, but most groups averaged tenures of less than five years.³⁵

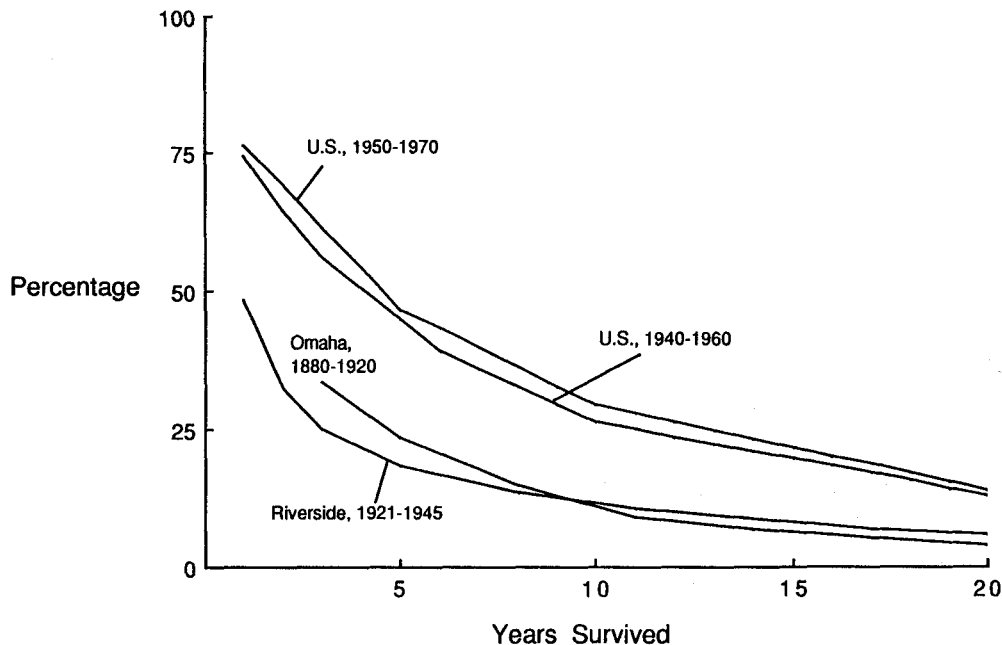
The only structural comparison of the interwar Riverside data we can make is with the information contained in the Public Use Sample of the 1940 census. Of 350,354 non-institutionalized heads of household in the sample, 43.5 percent owned their dwellings. Fully 59 percent of all heads of household did not live in the same house they had in 1935. Among persisters, who constituted 41 percent of the entire sample, 67.4 percent owned their homes, which again indicates the constraint home owning placed on residential mobility. The occupations of 158,449 heads reporting occupation, residence in 1935, and ownership status in 1940, further suggest that persistence was only marginally concentrated at the high end of the occupational scale. Only 46 percent of upper white-collar heads of household lived in the same house in 1940 as they had in 1935. Table 6 reveals high mobility among both owners and renters in all occupation groups and only a marginal propensity to own at the higher occupational levels.³⁶

³⁵ The mean lengths of tenure in years for Riverside homeowners and renters with known occupations are as follows:

<i>Occupational Group</i>	<i>All</i>		<i>Owners</i>		<i>Renters</i>	
	<i>Tenure</i>	<i>s</i>	<i>Tenure</i>	<i>s</i>	<i>Tenure</i>	<i>s</i>
High White Collar	5.2	6.2	7.2	7.0	2.3	2.0
Low White Collar	3.2	4.5	5.0	5.9	1.8	2.4
Skilled	3.4	4.7	5.2	5.8	1.9	2.7
Semi-Skilled	2.2	3.6	4.5	5.5	1.3	1.8
Unskilled	3.5	4.9	6.4	6.3	1.8	2.7
Eta	.21		.17		.14	

³⁶ Our brief comparison of the structural composition of Riverside's interwar population with that of the nation raises the important issue of whether we can compare the patterns of residential tenure in Riverside, and for that matter Omaha, Nebraska, with those of the country as a whole. Although Riverside's general social characteristics mirrored the nation's, as we discuss in the appendix, California and the West experienced substantial population growth. The net white migration rate in counties in the Pacific Region averaged 14 per thousand between 1930 and 1940, and 20.4 per thousand between 1940 and 1950. California counties alone experienced net migration rates of 21.3 and 25.8 per thousand during the same two decades. High positive net migration in California, however, does not axiomatically bias our analysis of residential mobility in Riverside, in part because other regions in the country had

Figure 1
Cumulative Proportions of Populations Persisting for Twenty Years,
Omaha, Nebraska, 1880–1920, Riverside, California, 1921–1950,
United States, 1940–1960, 1950–1970



SOURCES: Howard Chudacoff, *Mobile Americans: Residential and Social Mobility in Omaha, 1880–1920* (New York, 1972), 36; City of Riverside, Utility Billing Records, Riverside Municipal Archives, University of California, Riverside; Bureau of the Census, *Census of Population, 1960, Vol. 1: Characteristics of the Population, Number of Inhabitants, General Population Characteristics, General Social and Economic Characteristics, and Detailed Characteristics, Part 1, United States Summary* (1964), table 113, 1–258; and *idem*, *1970 Census of the Population, General Social and Economic Characteristics, United States Summary* (1972), table 150, 1–484.

These findings should come as no surprise. Historians have consistently found that home owning slows residential mobility and that occupational level (and, implicitly, income and social status) fails to discriminate well among patterns of residential tenure. Indeed, researchers of both historical and contemporary mobility often appeal to general, if not ubiquitous, characteristics of populations

high rates of negative net migration. Although substantial immigration may have contributed to lower rates of persistence in the West, overall postwar residential mobility nationwide differed minimally across regions when compared to pre-war levels. See Bureau of the Census, *1970 Census of the Population, General Social and Economic Characteristics, United States Summary* (1972), table 150, 1–484; and data compiled by John Gardner and William Cohen, "County-Level Demographic Characteristics of the Population of the United States, 1930–1950," from Kuznets and Thomas, eds., *Population Distribution and Economic Growth*; and D. Bogue, *Components of Population Change, 1940–1950*, available from the Inter-university Consortium for Political and Social Research (0020). Because high rates of net migration appear to affect residential mobility only marginally, at least regionally, we submit that the matter of Riverside's comparability, or typicality, ultimately rests on whether the Riverside data constitute an adequate sample of the larger, national pattern of residential mobility.

Samples are evaluated for their validity and reliability. A valid sample is one in which the behavior measured provides a meaningful indication of the underlying phenomenon being analyzed. While it would be hard to discredit utility billing records as a valid indicator of residential movement, whether

TABLE 6
Five-Year Persistence among Homeowners and Renters, by Occupation Group,
United States, 1940

<i>Owners and Renters</i>	<i>Total</i> <i>N</i>	<i>(%)</i>	<i>Persist</i> <i>N</i>	<i>(%)</i>
High White Collar	40,831	25.8	18,767	46.0
Low White Collar	28,102	17.7	10,377	36.9
Skilled	19,349	12.2	7,906	40.9
Semi-Skilled	34,036	21.5	12,641	37.1
Unskilled	36,131	22.8	13,300	36.8
Totals	158,449	100.0	62,991	39.8
<i>Owners</i>	<i>Total</i> <i>N</i>	<i>(%)</i>	<i>Persist</i> <i>N</i>	<i>(%)</i>
High White Collar	21,438	52.5	14,065	65.6
Low White Collar	11,009	39.2	6,849	62.2
Skilled	8,860	45.8	5,452	61.5
Semi-Skilled	12,385	36.4	7,625	61.6
Unskilled	12,599	34.9	8,102	64.3
Totals	66,291	41.8	42,093	63.5
<i>Renters</i>	<i>Total</i> <i>N</i>	<i>(%)</i>	<i>Persist</i> <i>N</i>	<i>(%)</i>
High White Collar	19,393	47.5	4,702	24.2
Low White Collar	17,093	60.8	3,528	20.6
Skilled	10,489	54.2	2,454	23.4
Semi-Skilled	21,651	63.6	5,016	23.2
Unskilled	23,532	65.1	5,198	22.1
Totals	92,158	58.2	20,898	22.7

SOURCE: The data used in Table 7, the *Census of Population, 1940 [United States]: Public Use Microdata Sample*, were originally collected by the U.S. Department of Commerce, Bureau of the Census, and were made available by the Inter-university Consortium for Political and Social Research. Neither the collector of the original data nor the Consortium bear any responsibility for the analyses or interpretations presented here.

when they attempt to explain mobility, in large part because particular social-structural characteristics possess only modest explanatory power. In short, virtually all work on the subject of historical and modern residential mobility points to causal mechanisms that all populations experience generally. Because we have no compelling empirical or theoretical reasons to believe that Riverside's particular social and economic characteristics substantially distort the picture of residential mobility that our analysis reveals, we turn to endemic causal forces.

our particular sample population possesses biases that fail to capture important underlying influences on residential mobility is another matter. Our necessarily sketchy analysis of socioeconomic characteristics of Riverside's population failed to reveal any social-structural differences between its renting and home-owning segments. At the same time, we did not exhaust all the possibilities, and thus we cannot say unequivocally that Riverside's social, economic, or ecological characteristics did not differ so markedly from other urban areas in the United States as to render our analysis of residential mobility unrepresentative of the nation. At the very least, we have provided a case study of interwar residential mobility.

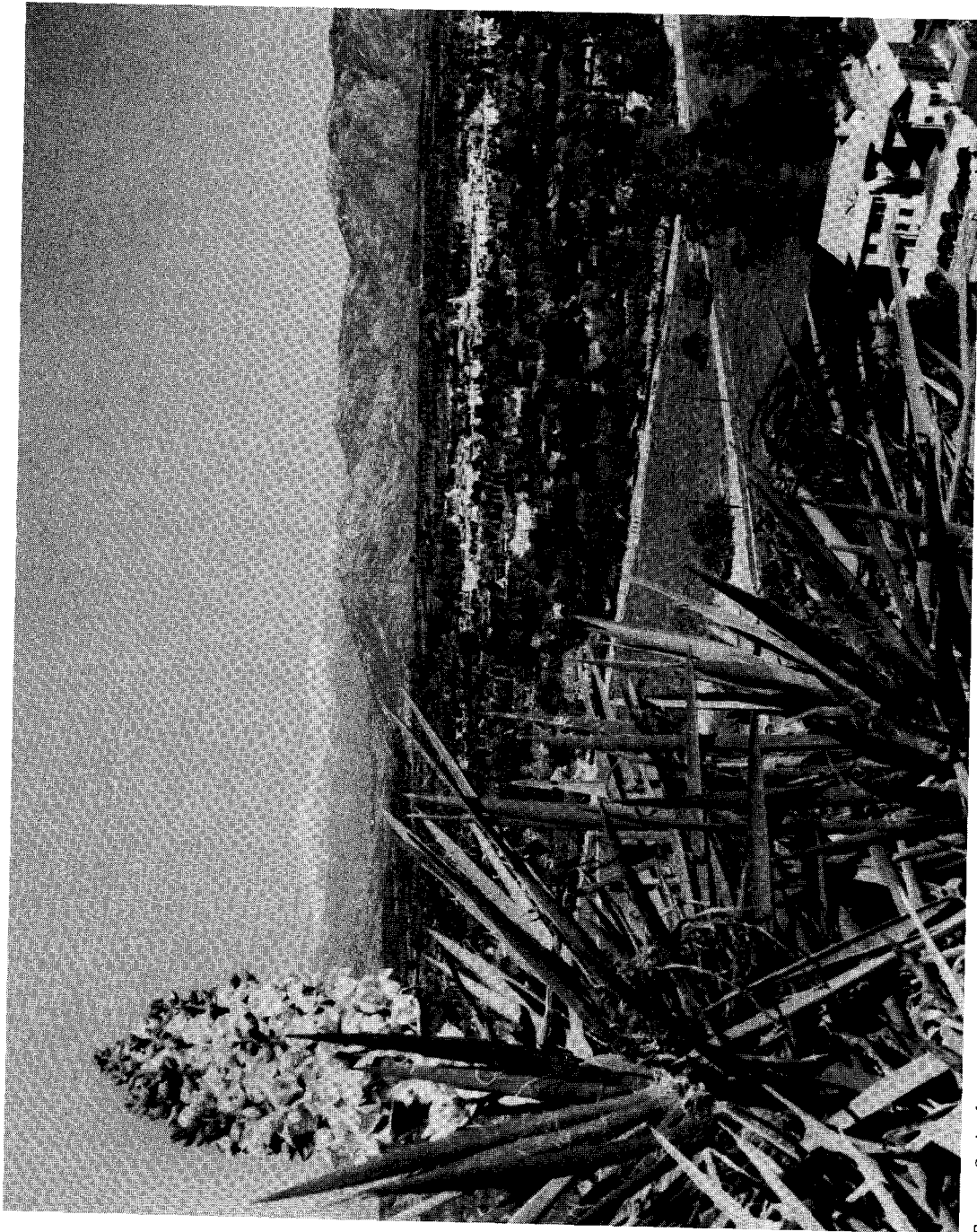


FIGURE 2: A photograph by Avery Edwin Field, *circa* 1935, "Riverside from Mount Rubidoux," courtesy of the Mission Inn Foundation.

SOCIAL SCIENTISTS HAVE DEVELOPED TWO EXPLANATIONS of postwar mobility. One, which locates the driver of residential mobility in the family life cycle, is explicit; the other, derived from economic analyses of consumption, is implicit. Neither alone seems capable of accounting for the magnitude of the change or the speed with which it occurred. An explanation can, however, be found in the housing policies of Franklin D. Roosevelt's New Deal state, which were themselves inspired by a revitalized Lockean-Jeffersonian conception of propertied citizenship first articulated by Herbert Hoover in the context of national housing needs and then embraced by Roosevelt and implemented with the power of the state.

Geographers and sociologists have increasingly come to believe that social acquaintance neighborhoods and community social structures depend less on socioeconomic status than on duration of residence. Although urban sociologists admit that the causes of residential mobility are "imperfectly understood," they agree that family life-cycle developments account for most of the reasons why people change residence.³⁷ Economic reasons for changing residence find expression in decisions regarding housing type and tenure status but invariably within the context of life-cycle needs. Rapid change in the racial composition of a neighborhood, for instance, will or will not generate sufficient dissatisfaction with current housing to impel a household to move, depending on a family's stage in the life cycle and the household's tenure status.³⁸ The longer a household maintains a single residence, the less important life-cycle and socioeconomic influences become in building social networks and community ties. This finding may explain why historical studies of modern urban social structure find socioeconomic factors to be weaker predictors of community behavior, such as electoral participation, than residential variables.³⁹ Exogenous, socioeconomic influences can still intervene, but, within shifting clusters of socioeconomic and life-cycle influences, tenure status and the characteristics of housing are consistently the most important determinants of residential mobility.

Two considerations should caution historians against using contemporary life-cycle perspectives on postwar intra-urban residential mobility to explain historical residential mobility. First, sociologists use the family life cycle on the assumption that they are explaining high rates of mobility, not low ones. In historical perspective, the life-cycle hypothesis encounters difficulties. Structurally, it is true, the American family did change at the same time residential mobility declined.

³⁷ Paul Knox, *Urban Social Geography: An Introduction*, 2d edn. (New York, 1987), 171, 162, 174, 177, 180, 183–84. See also Rossi and Shlay, "Residential Mobility and Public Policy Issues," 25–26; Earl W. Morris and Mary Winter, "A Theory of Family Housing Adjustment," *Journal of Marriage and Family*, 37 (1975): 79–88; Morris and Winter, *Housing, Family and Society* (New York, 1978); Charles F. Hohm, "Housing Aspirations and Fertility," *Sociology and Social Research*, 68 (1984): 350–63; Alden Speare, Jr., "Homeownership, Life Cycle Stage and Residential Mobility," *Demography*, 7 (1970): 449–58; A. Chevan, "Family Growth, Household Density and Moving," *Demography*, 8 (1971): 451–58; Alden Speare, Jr., Sidney Goldstein, and William H. Frey, *Residential Mobility, Migration, and Metropolitan Change* (Cambridge, Mass., [1975]), 233–48. Frey further discussed the life-cycle hypothesis in "Mover's Life-Cycle Stage and Choice of Destination Neighborhood: Implications for Urban Social Structure," in W. A. V. Clark and Eric G. Moore, eds., *Population Mobility and Residential Change* (Evanston, Ill., 1978), 99–148; see also Daniel R. Fredland, *Residential Mobility and Home Purchase: A Longitudinal Perspective on the Family Life Cycle and the Housing Market* (Lexington, Mass., 1974).

³⁸ Studies of Detroit in the early 1960s, when the racial composition of many neighborhoods was rapidly shifting, showed that the propensity to move depended most on whether households owned or rented their homes. See Eleanor Paperno Wolf and Charles N. Lebeaux, with Shirley Terreberry and Harriet Saperstein, *Change and Renewal in an Urban Community: Five Case Studies of Detroit* (New York, 1969), 21, 38, 93, 95.

³⁹ Knox, *Urban Social Geography*, 289–301, provides a convenient introduction to contemporary issues of political and para-political participation.

Postwar birth rates rose quickly and dramatically above pre-war levels. During the depression, the birth rate averaged 19.1, while postwar rates hovered around 25 until 1960, when the baby boom began to subside.⁴⁰ Logically, however, from the life-cycle perspective, a higher birth rate would raise the rate of residential change, not lower it, because families would need to acquire more rooms (or specialized rooms) or relocate in order to gain access to schools. Second, historical demographic trends weaken the family life-cycle explanation. Since 1840, the birth rate and family size have declined steadily. The rise in the birth rate and the corresponding increase in average family size after World War II were an aberration. If residential mobility paralleled long-term trends, it should have declined gradually from the mid-nineteenth century to 1945 and then risen sharply—yet it did not.

A second, implicit explanation of contemporary residential mobility lies in economic analyses of consumption. Economists of post-World War II consumer behavior argue that a basic division in spending patterns exists between homeowners and renters.⁴¹ The extent of homeownership rose at approximately the same time that the rate of residential mobility fell. Accordingly, one might infer that the increase in homeownership caused the decline in residential mobility. Post-1947 surveys show a strong relationship between homeownership and tenure. Nonetheless, our analysis does not support a relationship between homeownership and long residential tenure that is strong enough to explain, by itself, the dramatic decline in residential mobility after 1945. Before World War II, owners had longer average residential tenure than renters, but both owners and renters moved often. In short, no unequivocal causal connection exists between the simple fact of homeownership and a decrease in residential mobility.

It seems that the sudden drop in residential mobility after 1950 was caused by changes in the conditions of home buying that led to the increase in homeownership. We suggest, moreover, that the federal government deliberately changed the conditions of home buying in order to decrease geographic mobility in the United States. Between 1933 and 1937, federal legislation established mechanisms to regulate the banking and securities industry, rescue banks and financial institutions involved in the housing market by buying up defaulted mortgages, lend directly to home buyers for long terms, encourage private lending by insuring mortgages, initiate public housing and urban redevelopment programs, stimulate the construction industry, regulate the private power industry that supplied residential dwellings with electricity, and modernize the nation's older housing stock.⁴² Following

⁴⁰ *Historical Statistics*, Series B, 5, 1: 49. The mean birth rate from 1931 through 1941 was 19.1 ($s = 0.7$); from 1946 through 1959, it was 24.0 ($s = 0.7$).

⁴¹ See, for instance, Carolyn Bell, *Consumer Choice in the American Economy* (New York, 1967), 64–65. Bell also noted that federal intervention in home financing stimulated the shift from renting to owning homes. For an economic model of the home-buying decision, which assumes the home buyer is a rational investor, see Wallace F. Smith, *Housing: The Social and Economic Elements* (Berkeley, Calif., 1976), 321–65. Smith's model takes into consideration the effect of tax policies and tax liability in the consumer's decision.

⁴² On housing and home financing in the New Deal, see Henry J. Aaron, *Federal Housing Subsidies: History, Problems, and Alternatives* (Washington, 1973); Joseph L. Arnold, *The New Deal in the Suburbs: A History of the Greenbelt Town Program, 1933–1954* (Columbus, Ohio, 1971); Henry C. Bredemeier, *The Federal Public Housing Movement: A Case Study of Social Changes* (1955; rpt. edn., New York, 1980); Paul K. Conkin, *Tomorrow a New World: The New Deal Community Programs* (Ithaca, N.Y., 1959); Gertrude S. Fish, "Housing Policy in the Great Depression," in Fish, ed., *The Story of Housing* (1979), 177–241; C. Lowell Harris, *History and Policy of the Home Owners' Loan Corporation* (Washington, 1951); Kenneth T. Jackson, *Crabgrass Frontier: The Suburbanization of the United States* (New York, 1985); Nathaniel S. Keith, *Politics and the Housing Crisis since 1930* (New York, 1973); Timothy McDonnell, *The Wagner Housing Act*

the war, housing programs for veterans reinforced New Deal initiatives to reorder home financing completely. The state accomplished this with unparalleled speed. In 1935, the state, through the FHA, insured only 6 percent of all private mortgages. In 1950, the FHA and the Veterans' Administration (VA) together insured half of all new home mortgages.⁴³

Federal initiatives slowed residential mobility by lengthening the amortization period of the home mortgage and so made home buying easier and more attractive. In the 1920s, the terms of home mortgages averaged five to ten years and frequently called for repayment in as few as three years. In California in the 1920s, most mortgage periods were short. A survey of first-mortgage lenders in San Diego, Los Angeles, and San Francisco showed that 65.7 percent of all mortgages had repayment periods of five years or less, another 27.5 had periods of between seven and twelve years, and only 6.8 percent had terms of fifteen years.⁴⁴ During the depression, banks saw their existing assets dwindle at the same time that they found new ones difficult to acquire. Not only did defaults place a burden on banks but older, short-term mortgages were being completed at a time when few new loans were being written. Short-term mortgages meant high monthly payments, which made home buyers vulnerable to future hard times and reluctant to take on debt. The FHA altered the situation by lengthening the amortization period to twenty years and insuring mortgages. At the same time that the FHA lowered homeowners' monthly payments, they assured banks of steady, long-term income at virtually no risk. By 1940, the average FHA-insured mortgage for single-family residences had an amortization period of twenty-three years.⁴⁵ As the FHA and VA insured an increasing number of home loans following the war, lenders not participating directly in the federal programs also issued longer mortgages in order to compete. The net result: nearly all postwar mortgages were twice as long as pre-war ones.

Longer mortgages cost individuals more interest than they would have paid on loans of equal value with shorter terms, despite lower monthly payments. Although

(Chicago, 1957); Robert Moore Fisher, *Twenty Years of Public Housing* (New York, 1959); and Mark I. Gelfand, *A Nation of Cities: The Federal Government and Urban America, 1933–1945* (New York, 1975).

⁴³ *Housing and Home Finance Agency, Sixth Annual Report, Calendar Year 1952* (Washington, 1953), 115. Assessing the overall effect of FHA and Veterans' Administration policies on the nation's housing stock remains, at this point, impossible. However, published data exists on FHA involvement in 122 standard metropolitan areas (SMAs) between 1935 and 1950 that contained 62.4 percent and 61.3 percent of all occupied housing units in the country in 1940 and 1950, respectively. In 1940, FHA mortgages (including both Title I mortgages on existing homes and Title II mortgages on new housing units) had been applied to an average of 3.3 percent ($s = 2.1$) of the total housing stock in the 122 SMAs. Moreover, on average, 18.6 percent ($s = 9.2$) of new housing constructed between 1940 and 1950 in the 122 SMAs was FHA insured, which raised the level of FHA's average penetration in the housing stock in 1950 to 10.1 percent ($s = 5.3$). Federal Housing Administration, *FHA Homes in Standard Metropolitan Areas, 1950: New England Division, Middle Atlantic Division* (Washington, 1952); *FHA, FHA Homes in Standard Metropolitan Areas, 1950: South Atlantic Division* (Washington, 1952); *FHA, FHA Homes in Standard Metropolitan Areas, 1950: East North Central Division* (Washington, 1952); *FHA, FHA Homes in Standard Metropolitan Areas, 1950: West North Central Division* (Washington, 1952); *FHA, FHA Homes in Standard Metropolitan Areas, 1950: East South Central Division, West South Central Division* (Washington, 1952); *FHA, FHA Homes in Standard Metropolitan Areas, 1950: Mountain Division, Pacific Division* (Washington, 1952); and *Historical Statistics*, Series N, 236, 2: 646. For an overview of federal housing policies, see Wendt, *Housing Policy—The Search for Solutions*, 145–229.

⁴⁴ John M. Gries and James Ford, eds., *The President's Conference on Home Building and Home Ownership, Home Finance and Taxation, Reports of the Committees on Finance, Frederik H. Ecker, Chairman, Taxation, Thomas S. Adams* (Washington, 1931), 56.

⁴⁵ "Characteristics of Mortgages, Homes, and Mortgagors: Based on FHA-Insured Mortgages Secured by New and Existing Single-Family Homes, Sec. 203, for Selected Years 1940–48," *Housing and Home Finance Agency, Second Annual Report: 1949* (Washington, 1950), 194.

interest had been a tax deduction since 1913, neither the amount of interest nor the amount of federal tax was sufficiently burdensome before 1941 to make the interest deduction a reason in itself for households to shift from renting to owning. World War II dramatically changed the situation by driving up tax liability. For a household with four exemptions and a net income of \$5,000, actual tax liability jumped from 1.5 percent in 1940 to 15.1 percent in 1945. Although postwar tax liability declined from its 1945 peak, the necessity of paying off the war debt and funding cold-war budgets kept it at approximately 10 percent until 1964. The interest deduction thus sheltered a larger portion of income and so provided both an incentive and reward for buying a home.⁴⁶

Long-term mortgages lengthened the time it took to accumulate equity. Because amortization schedules required repayment of principal faster than dwellings depreciated, homeowners did build equity over time, but it was a slow process. High interest payments early in the loan period reduced the net return (if any) from an early sale. The long-term mortgage thereby imposed a financial disincentive to sell soon after buying and, as a practical matter for most households, a disincentive to move. From this perspective, the increase in residential persistence would naturally parallel the increase in the number of long-term mortgages, which were written under federal guarantees or the influence of federal policy.⁴⁷ But it is important to see the thinking behind this change in federal policy.

AS PART OF A SOLUTION TO THE LONG-NOTICED AND MUCH-DISCUSSED PROBLEM of social cohesion in the American body politic, New Deal housing programs sought to increase residential stability by increasing homeownership. In the late nineteenth century, social reformers perceived homeownership as an important element in improving housing conditions, reforming the social life of cities, and strengthening the bonds of citizenship between Americans and their government. Social conservatives arrived at similar conclusions, although from dissimilar assumptions. Conservatives were concerned with protecting the social order of capitalism by thwarting the rise of radicalism among the poor and laboring classes. Homeownership, especially of suburban homes, promised to stabilize the labor supply, to tie working peoples' sympathies to the order of capital rather than to unions, and to

⁴⁶ *Historical Statistics*, Series Y, 430, 2: 1112. The standard source on the history of taxation is Randolph E. Paul, *Taxation in the United States* (Boston, 1954), but see also Constance Perin, *Everything in Its Place: Social Order and Land Use in America* (Princeton, N.J., 1977), 70. The proportion of federal tax returns claiming interest deductions rose from 2.8 percent in 1950 to 30.6 percent in 1960, and to 39.3 percent in 1970. U.S. Department of the Treasury, Internal Revenue Service, *Statistics of Income for 1950*, Part I (Washington, 1954), 45; *Statistics of Income, 1960: Individual Income Tax Returns* (Washington, 1962), 55; *Statistics of Income, 1970: Individual Income Tax Returns* (Washington, 1972), 103. Irving Welfeld argued, contrary to our thesis, that the tax deduction for mortgage interest could not have been a significant factor pushing homeownership because "most families did not earn enough money to make the tax break attractive; even in the 1980s, three quarters of homeowners took the standard deduction"; Welfeld, *Where We Live: A Social History of American Housing* (New York, 1988), 57–58.

⁴⁷ See Bell, *Consumer Choice*, 64–65. For a discussion of the contemporary impact of FHA policies, see Perin, *Everything in Its Place*, *passim*. The distinction between home buying and homeownership is fundamental to Perin's thesis that home buying under the long-term mortgage contract, with the establishment of the condition of permanent debtor for the home buyer, represents "the ideal of perfected citizenship." Perin observed this phenomenon in the 1970s and was not making a historical examination of its origin; 72–73. Forced savings, in the form of purchase of equity, was typical of capital consumption credit after the federal reorganization of the credit markets in the 1930s. See Rolf Nugent, *Consumer Credit* (New York, 1939).

bring conservative civic participation.⁴⁸ Although the Jeffersonian Republican vision of a nation of rural, small property owners had fallen before the Jacksonian mandate of universal white manhood suffrage, reformers contemplated the propertyless immigrant masses in American cities and expressed concern that the lack of real property ownership weakened civic cohesion. Although no prominent reformer sought real property ownership as a requirement for voting, homeownership emerged as an indirect means of rebuilding citizenship, and civic participation generally, on a conservative social basis.

In line with a conservative version of progressivism, Herbert Hoover stressed the importance of homeownership to the nation during his terms as secretary of commerce. With the collapse of the housing market after 1927 and the stock market crash of 1929, Hoover's concern escalated to alarm. He called the Conference on Home Building and Home Ownership in 1931 to review all aspects of the nation's housing. Homeownership, as the title of the conference emphasized, was perceived as the key to improving standards of living and lowering social pathology in the cities. It was also the key to the revival of citizenship. Hoover's conferees specifically recommended lengthening the terms of home mortgages to protect homeowners from the depression and to stabilize property values. Less specifically, but equally important, they expressed concern about the high rate of residential mobility—or what they perceived to be residential instability—when they dealt with the ideal neighborhood of single-family homes in an organic community. They assumed, along with sociologists of the period, that residential mobility harmed the body politic. Hoover himself placed the issue of home owning squarely in the context of citizenship when he said that home owning “makes for better citizenship.”⁴⁹

To Franklin Roosevelt, the Great Depression threatened homeownership and the state itself. Roosevelt perceived the failure of the American economic and political system to protect private property in the home as a central problem of the depression. As both presidential candidate and New Deal president, Roosevelt expressed a consistent vision of the centrality of private homeownership in a stable American social order. He also believed that the social compact of the early Republic, which Jefferson had defined in terms of the yeoman farmer who worked the land he owned, needed to be modernized for the industrial era. In a campaign

⁴⁸ See David P. Handlin, *The American Home: Architecture and Society, 1815–1915* (Boston, 1979), 235–69; Jackson, *Crabgrass Frontier*, 50–51; Daniel D. Luria, “Wealth, Capital, and Power: The Social Meaning of Home Ownership,” *Journal of Interdisciplinary History*, 7 (1976): 261–82; Matthew Edel, Elliot D. Sclar, and Daniel Luria, *Shaky Palaces: Homeownership and Social Mobility of Boston's Suburbanization* (New York, 1984), 171–91; and Carolyn Tyirin Kirk and Gordon W. Kirk, Jr., “The Impact of the City on Home Ownership: A Comparison of Immigrants and Native Whites at the Turn of the Century,” *Journal of Urban History*, 7 (1981): 471–98. The context of household modernization is provided by Gwendolyn Wright, *Building the Dream: A Social History of Housing in America* (New York, 1981), 96–113; 158–76. For a contemporary introduction to the problem, see Helen Campbell, *Household Economics: A Course of Lectures in the School of Economics of the University of Wisconsin*, rev. edn. (1896; rpt. edn., New York, 1907); and Ellen H. Richards, *The Cost of Shelter* (New York, 1905). James Kloppenberg discussed the development of social democratic theory within which the problem of social cohesion in a society of largely propertyless citizens is developed by theorists such as John Dewey; Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920* (New York, 1986).

⁴⁹ Robert B. Fairbanks traced the development of the theme of better citizenship among Cincinnati's housing reformers in the rise of urban planning in that city in the 1920s; Fairbanks, *Making Better Citizens: Housing Reform and the Community Development Strategy in Cincinnati, 1890–1960* (Urbana, Ill., 1988), 25–37, 41–57, 104–13. See also Dolores Hayden, *Redesigning the American Dream: The Future of Housing, Work, and Family Life* (New York, 1984), 32–38. For Hoover's remarks, see Gries and Ford, *President's Conference on Home Building and Home Ownership*, 2.

speech before the Commonwealth Club of San Francisco in September 1932, Roosevelt espoused a modernized social compact grounded in secure savings. "Every man has a right to his own property; which means a right to be assured, to the fullest extent attainable, in the safety of his savings. By no other means can men carry the burdens of those parts of life which, in the nature of things, afford no chance of labor; childhood, sickness, old age. In all thoughts of property, this right is paramount."⁵⁰

As president, Roosevelt left no doubt that he included homeownership as a centerpiece of a modernized social compact. Securing that goal, however, required the national government to intervene directly in the home financial market. Mortgages on single family homes constituted the "backbone of the American financial system."⁵¹ "The broad interests of the Nation," the president argued to Congress in asking for the Home Owners' Loan Act in 1933, "require that special safeguards should be thrown around home ownership as a guarantee of social and economic stability."⁵² Roosevelt's ability to wield the power of the state to implement his policies and achieve his goals separated him from earlier advocates of the idea. The Home Owners' Loan Corporation, established in June 1933, purchased defaulted mortgages and refinanced them for terms of fifteen years. In existence for only three years, the program refinanced 1,021,587 loans.⁵³ While the Home Owners' Loan Act protected existing homeowners, the Federal Housing Administration, established by the National Housing Act of 1934, was the cornerstone of Roosevelt's program. The FHA sought to extend homeownership more broadly to the American people by insuring long-term amortized mortgages written by private lenders, abolishing second mortgages, lowering interest rates, and standardizing loan practices.⁵⁴

A concrete, anecdotally passionate vision of health, security, and physical comfort in modernized housing propelled Roosevelt's commitment to New Deal housing legislation. He believed, and preached to the American people, that personal health and security in one's own home, conceived of as a dwelling of long-term residence that met minimum standards of physical shelter and hygiene, was a fundamental right in the modernized state.⁵⁵ While the full history of New

⁵⁰ Franklin D. Roosevelt, "'New Conditions Impose New Requirements upon Government and Those Who Conduct Government,' Campaign Address on Progressive Government at the Commonwealth Club, San Francisco, Calif., September 23, 1932," in Samuel I. Rosenman, comp., *The Public Papers and Addresses of Franklin D. Roosevelt*, 13 vols. (New York, 1938–50), 1: 754; hereafter, FDR, *Public Papers*. The background of Roosevelt's Jeffersonianism is provided by Daniel R. Fusfeld, *The Economic Thought of Franklin D. Roosevelt and the Origins of the New Deal* (New York, 1956), 82–89, 98–100, 242–44.

⁵¹ "Campaign Address on the Eight Great Credit Groups of the Nation, St. Louis, Mo., October 21, 1932," FDR, *Public Papers*, 1: 829.

⁵² "Message Asking for Legislation to Save Small Mortgages from Foreclosure, April 13, 1933," FDR, *Public Papers*, 2: 135.

⁵³ "The Home Owners Loan Act Is Signed—The President Urges Delay in Foreclosures, June 13, 1933," FDR, *Public Papers*, 2: 233–37.

⁵⁴ "Recommendation for Legislation to Provide Assistance for Repairing and Construction of Homes, May 14, 1934," FDR, *Public Papers*, 3: 232–36. See also "The One Hundred and Sixty-first Press Conference (Excerpts), Warm Springs, Ga., November 28, 1934," *ibid.*, 3: 479–84; "Address before the American Country Life Conference on the Better Distribution of Population Away from Cities, Ithaca, N.Y., August 19, 1931," 1: 505–06; "The Philosophy of Social Justice through Social Action, Campaign Address at Detroit, Mich., October 2, 1932," 1: 774–75; "Campaign Address on the Eight Great Credit Groups of the Nation, St. Louis, Mo., October 21, 1932," 1: 820; and "Proceedings of the National Emergency Council [Meeting no. 19], December 11, 1934," in Lester G. Seligman and Elmer E. Cornwell, Jr., eds., *New Deal Mosaic: Roosevelt Confers with His National Emergency Council* (Eugene, Ore., 1965), 365–68.

⁵⁵ The first generation of Roosevelt scholars did not perceive a vision of housing as a significant feature of his social philosophy. James MacGregor Burns did not focus on Roosevelt's interest in

Deal housing programs, including an appreciation of the president's vision of the home, remains to be written, Roosevelt's public addresses in the 1930s clearly indicate the importance of the residentially stable home to his social philosophy. In his message to Congress in June 1934, made in part to support the National Housing Act then being considered, the president reviewed the great objectives of the New Deal, emphasizing "the security of the home, the security of livelihood, and the security of social insurance . . . a minimum of the promise that we can offer to the American people."⁵⁶

Roosevelt explicitly sought residential stability as a policy goal for farmers. In his review of the New Deal in June 1934, Roosevelt sketched the nation's history in order to emphasize the thesis that the historical opportunity for Americans to cope with adversity by emigration had passed. In a modern, industrialized nation, families were unable to move; better lives had to be attained where they currently lived. Reconstructing American society involved creating the conditions in which residential longevity in the same home was possible:

In a simple and primitive civilization homes were to be had for the building. The bounties of nature in a new land provided crude but adequate food and shelter. When land failed, our ancestors moved on to better land. It was always possible to push back the frontier, but the frontier has now disappeared. Our task involves the making of a better living out of the lands that we have . . . In pursuing this policy we are working toward the ultimate objective of making it possible for American families to live as Americans should.⁵⁷

Thematic references to security and stability in the home recurred in Roosevelt's speeches and addresses between 1935 and 1938, as he fought for social security and public housing.

The Resettlement Program approached residential stability in a roundabout way. Farm families that could not make a living on submarginal lands were to be resettled on better land where they and future generations could pursue agriculture profitably.⁵⁸ Roosevelt explicitly stated his goal of long-term residential stability for farm families in a message to Congress in 1937: "[M]any tenants change farms every two or three years, and apparently one out of three changes farms

housing or the home, except to use an anecdote to illustrate Roosevelt's practicality; Burns, *Roosevelt: The Lion and the Fox* (New York, 1956), 245. Thomas H. Greer did not treat Roosevelt's concern for the stability of the home or the relationship between residential stability and citizenship; Greer, *What Roosevelt Thought: The Social and Political Ideas of Franklin D. Roosevelt* (East Lansing, Mich., 1958), 14–15, 142–49. Unlike many of his contemporaries, Mario Einaudi did perceive Roosevelt's support for better housing as a reflection of a social vision and not simply economic relief, but he approached the issue through social security; Einaudi, *The Roosevelt Revolution* (New York, 1959), 87–90. Arthur M. Schlesinger did not treat the National Housing Act and mentioned the FHA only in passing; Schlesinger, *The Age of Roosevelt*, 3 vols. (Boston, 1957–60). William E. Leuchtenburg also perceived the president's interest in housing programs as a means of economic revival; Leuchtenburg, *Franklin Roosevelt and the New Deal, 1933–1940* (New York, 1963), 134–36. Part of this general impression is undoubtedly due to the Roosevelt administration's belated commitment to public housing and the failure of the progressive housing reform movement to obtain direct national regulation of housing conditions during the New Deal. See Harry Charles Bredemeier, *The Federal Public Housing Movement: A Case Study of Social Change* (New York, 1980), 74–122. Richard Davies canonized this historiography in his account of President Truman's strong support of public housing; Davies, *Housing Reform during the Truman Administration*, 29.

⁵⁶ "Message to the Congress Reviewing the Broad Objectives and Accomplishments of the Administration," June 8, 1934, FDR, *Public Papers*, 3: 292.

⁵⁷ "Message to the Congress Reviewing the Broad Objectives and Accomplishments," 3: 288–89. Roosevelt often repeated the ideological theme of security and stability; see, for example, "Annual Message to Congress, January 4, 1935," *ibid.*, 4: 17.

⁵⁸ "The Resettlement Administration Is Established," Executive Order No. 7027, May 1, 1935, FDR, *Public Papers*, 5: 143, 144 n.

every year. The agricultural ladder, for these American citizens, has become a treadmill."⁵⁹ When Roosevelt wished to remind his political audience of the full horror of the depression, he painted a picture of homeless, unemployed people in which the supreme importance of a stable home was emphasized by its loss.⁶⁰ In a radio address, his commitment to stability appeared where one might expect it, in a discussion of children. "We are concerned about . . . children who are without adequate shelter or food or clothing because of the poverty of their parents. We are concerned about the children of migratory families who have no settled place of abode or normal community relationships."⁶¹

As European nations moved toward war, Roosevelt recalled his famous 1932 Commonwealth Club address to reiterate the theme that economic security, including homeownership, was necessary for a democratic society. Historical events turned the barrel of the social kaleidoscope, and the familiar elements of the mosaic at which the president looked fell into a new picture. Now he could see clearly that a stable home was the basis for more than the normal community relations that were every American's birthright. The stable, secure home was the basis for a democratic citizenship. "History proves that dictatorships do not grow out of strong and successful governments, but out of weak and helpless ones. If by democratic methods people get a government strong enough to protect them from fear and starvation, their democracy succeeds; but if they do not, they grow impatient."⁶²

European war erupted in September 1939. Invasions and combat inevitably destroyed homes and displaced vast populations. Roosevelt's fear for the security and stability of the home deepened, and he made the most emotional statement of his social vision. Against the background of the new war in Europe, he urged support of the Community Chest campaign. Roosevelt's impassioned plea touched many themes and images that he had developed in previous years:

Let us, as we sit in our homes tonight, give thought to some of our less fortunate fellow Americans who live in homes less cheerful than ours, homes often on the border line of poverty, misery and privation. And let us not forget that it is just as important to keep the lamp of hope burning in our more humble homes as it is to maintain the elaborate establishments in which abundance and even luxury are the rule. It is the survival of the old spirit that the home must be guaranteed. For the family still remains the basis of society as we know it, and it must be preserved as an institution if our democracy is to be perpetuated. If we lose the home we are in grave risk of undermining all those other elements of stability and strength which contribute to the well-being of our national life.⁶³

The depression loaded Roosevelt's political rhetoric with negative imagery of mobility. Veterans marching across the heartland for their bonuses in 1932, families thrown out of their homes and off their farms by foreclosure, vagrants drifting across the nation and clustering briefly in Hooverilles only to disperse days later, unemployed men leaving their families behind and moving from city to

⁵⁹ "A Message to the Congress on Farm Tenancy, February 16, 1937," FDR, *Public Papers*, 6: 81.

⁶⁰ See, for example, "Campaign Address at Kansas City, Mo., 'America Will Have to Be Led in the Days to Come by the Youth of Today,'" October 13, 1936, FDR, *Public Papers*, 5: 470.

⁶¹ "Address at the White House Conference on Children in a Democracy," April 23, 1939, FDR, *Public Papers*, 9: 244.

⁶² "Fireside Chat on Present Economic Conditions and Measures Being Taken to Improve Them," April 14, 1938, FDR, *Public Papers*, 7: 242-43. Earlier in this address, the president recalled previously enacted reform measures that he considered necessary for the economic security protecting democracy and liberties and that encouraged homeownership; *ibid.*, 7: 240.

⁶³ "Radio Address on Behalf of the Mobilization for Human Needs," October 9, 1939, FDR, *Public Papers*, 8: 535.

city looking for work, slum families evicted for failure to pay rent, and families too poor to support themselves separating into different public institutions: powerful, dramatic images of household instability that testified to the nation's failure to modernize its social compact. To produce a nation of stable households, Roosevelt led the New Deal coalition to support home buying with the power of the state and settle the American people in.

OUR STUDY OF RESIDENTIAL MOBILITY in interwar Riverside and the postwar United States yields several findings. First, the evidence on tenure in Riverside before 1950, and especially before 1946, carries implications for understanding both historical and postwar residential mobility. Historical estimates of 50 percent turnover within a decade probably mask a cycle of movement that was much shorter, on the average about two years. Also, interwar residential mobility paralleled the general pattern of nineteenth-century mobility historians have consistently found, which leads us to conclude that there was no fundamental change in the American propensity to move until after World War II. Second, the two major explanations advanced by postwar social scientists to explain contemporary residential mobility cannot by themselves account for the sudden, dramatic, and nationwide decrease in mobility that has characterized the United States since 1950. We believe that Franklin Roosevelt's New Deal housing programs and policies ultimately constrained mobility by making homeownership easier and more attractive than it had been since the mid-nineteenth century. Roosevelt's vision of a modernized social compact depended on a residentially stable citizenry. The size and power of the state that Roosevelt's administration created allowed the federal government to intervene directly and forcefully in the home-financing market to achieve the president's goal. It was not, however, simply a matter of the state providing access to home owning that restrained the American people's propensity to move but rather its providing the means, the long-term mortgage, which was motivated by the government's desire to promote better citizenship.

We recognize that documenting the change in residential mobility before and after World War II does not explain it. We have tried to suggest that variations in endemic socioeconomic characteristics do not appear powerful enough to explain the dramatic shift in the historic pattern that we have observed. Instead, any satisfactory explanation of the change in residential mobility must take into account the enormous role of the state. William Leuchtenburg recently argued that historians beset by myriad varieties of social history and calls for synthesis needed to resurrect the state as a major factor in American history.⁶⁴ We concur and further suggest that historians will need to reevaluate the role of Franklin Roosevelt's New Deal state in restructuring such a basic feature of American life as residential mobility, armed with social-scientific sensibilities that will allow them to analyze the political articulation of social values, the bureaucratic implementation of social policy, and the experience of ordinary people.⁶⁵

⁶⁴ William E. Leuchtenburg, "The Pertinence of Political History: Reflections on the Significance of the State in America," *Journal of American History*, 73 (1986): 585-600.

⁶⁵ Charles Tilly has issued a similar call to European social historians to start "(1) documenting large structural changes, (2) reconstructing the experiences of ordinary people in the course of those changes, and (3) connecting the two." Tilly, "Retrieving European Lives," in Olivier Zunz, ed., *Reliving the Past: The Worlds of Social History* (Chapel Hill, N.C., 1985), 31.

Appendix: A Description of Riverside, California

As the occupation, housing tenure, and migration statistics we have presented indicate, twentieth-century Riverside has been structurally similar and only marginally different from the nation's urban places generally.* Riverside also had racial and income diversity similar to the nation. Riverside's history followed the same shift from small town to metropolis that the nation experienced in the late nineteenth and early twentieth centuries.† Riverside's economy and the history of its housing stock also closely participated in the national economic trends. Tables A.1 and A.2 compare Riverside's population and housing stock to the nation in 1950 and indicate that differences are only of degree, not of kind.

Riverside's experience closely resembled that of other urban areas, especially its place in the urban hierarchy. The Census Bureau categorizes urban places into ten ranks. Over the history of the nation, the number of places in each class has changed. In 1850, there were many small towns, few large cities, and only one metropolitan area (New York) nearing a million people. In 1900, the dramatic increase in small towns testified to the settlement of the West, and more large cities evidenced the continued growth of historic eastern centers. Between 1920 and 1950, the urban hierarchy did not change as dramatically at the lower and upper ranks as it had previously. The most dramatic changes came in the ranks of small

*George William Beattie and Helen Pruitt Beattie, *Heritage of the Valley: San Bernardino's First Century* (1939; rpt. edn., Oakland, Calif., 1951), provides an introduction to the nineteenth-century history of the Riverside area. Riverside's standard local history is Tom Patterson, *A Colony for California: Riverside's First Hundred Years* (Riverside, 1971), which, true to its genre, does not reference its sources but which has proved to be reliable. Several scholarly publications treat Riverside's minority populations: Mark Rawitch, *No Other Place: Japanese American Frontiers in a Southern California Neighborhood* (Riverside, 1983); The Great Basin Foundation, *Wong Ho Lung: An American Chinatown* (San Diego, 1987); and Joyce Carter Vickery, *Defending Eden: New Mexican Pioneers in Southern California, 1830-1890* (Riverside, 1977).

†Riverside's population increased from 7,973 in 1890 to 29,696 in 1930 to 84,332 in 1960. Bureau of the Census, *Thirteenth Census of the United States Taken in the Year 1910*, Vol. 2: *Population, 1910, Reported by States* (Washington, 1913), 152; *idem*, *Sixteenth Census of the United States: 1940, Population*, Vol. 1: *Number of Inhabitants* (Washington, 1942), 34; *idem*, *United States Census of Population: 1960, Number of Inhabitants, California, Final Report PC(1)-6A* (Washington, 1961), 6-30; *idem*, *1980 Census of Population, Number of Inhabitants: California* (Washington, 1982), 6-14.

TABLE A.1
Population Characteristics, 1950, Riverside, California, and the United States

	Riverside	United States Rural and Urban
Population	46,764	
Median Age (Years)	32.0	30.2
Percent of Population 65 Years and Older	11.0	8.2
Percent of Population Nonwhite	4.9	10.5
Persons per Household	3.0	3.4
Families and Unrelated Individuals, Median Income (1950 Dollars)	\$ 2,857	\$2,619
Families and Unrelated Individuals, Percent with Incomes Less than \$2000	34.7	38.6

SOURCES: Bureau of the Census, *United States Census of Population: 1950*, Vol. 2: *Characteristics of the Population*, Part 5, *California* (Washington, 1953), table 10, 5-52; and Part 1, *United States Summary* (Washington, 1952), table 36, 1-88, table 37, 1-89, table 38, 1-91, table 44, 1-96, table 47, 1-97, table 57, 1-104, table 84, 1-136.

TABLE A.2
Housing Characteristics, 1950, Riverside, California, and the United States

	<i>Riverside</i>	<i>United States Rural and Urban</i>
Dwelling Units	16,056	
Age of Dwellings:		
Percent Built in Specified Years		
1945 or later	19.0	13.0
1940 to 1944	10.0	7.0
1930 to 1939	15.0	13.0
1920 to 1929	24.0	20.0
1919 or earlier	32.0	46.0
Percent of Dwellings with Central Heating	39.1	50.4
Cooking Fuel:	89.0	67.0
Percent of Dwellings Using Utility Gas or Electricity		
Percent of Dwellings with Television	19.0	12.0
Percent of Dwellings with Mechanical Refrigeration	86.4	80.2

SOURCES: Bureau of the Census, *United States Census of Population: 1950, Census of Housing, United States Summary* (Washington, 1953), table 20, 1-56; Vol. 1: *General Characteristics*, Part 2, table 20, 5-46.

cities, as towns grew in size and jumped classifications. Riverside's growth typified this group of cities. Put somewhat differently, from 1900 to 1950, Riverside grew through the urban ranks that contained about one-third of the urban population. As Table A.3 reveals, this growth put the city squarely in the middle of characteristic urban growth in this century, giving us further confidence that its institutional, social, and demographic characteristics were representative of the nation's urban experience in these years.[‡]

[‡]Ronald C. Tobey, "How Urbane Is the Urbanite? An Historical Model of the Urban Hierarchy and the Social Motivation of Service Classes," *Historical Methods Newsletter*, 7 (1974): 259-75.

TABLE A.3
Number and Proportion of Urban Areas, by Size, 1850-1950

<i>City Size</i>	<i>1850</i>		<i>1900</i>		<i>1920</i>		<i>1950</i>	
	(N)	(%)	(N)	(%)	(N)	(%)	(N)	(%)
2,500-4,999	89	39.2	832	9.6	1255	8.1	1570	6.3
5,000-9,999	85	36.4	465	10.6	715	9.2	1133	8.9
10,000-24,999	36	15.9	280	14.4	465	13.0	814	14.0
25,000-49,999	16	7.0	82	9.3	143	9.4	271	8.4
500,000-999,999	1	0.4	3	5.5	9	11.5	13	10.3
1,000,000+	0	0.0	3	21.3	3	18.7	5	19.6

Riverside's rank is in italics.

SOURCES: "Number of Places in Urban and Rural Territory, by Size of Place: 1790 to 1970," Bureau of the Census, *Historical Statistics of the United States*, 2 vols. (Washington, 1975), Series A 57-72, 1: 11.

Natural Frontiers Revisited: France's Boundaries since the Seventeenth Century

PETER SAHLINS

UNTIL ABOUT FIFTY YEARS AGO, the idea of France's natural frontiers was a commonplace in French history textbooks and in scholarly inquiry into Old Regime and revolutionary France. The idea, as historian of the revolution Albert Sorel wrote in 1885, was that "geography determined French policy": that, since the sixteenth, if not the twelfth, century, France had undertaken a steady and consistent expansion to reach the Atlantic, Rhine, Alps, and Pyrenees.¹ These were "the limits that Nature has traced," which Cardinal Richelieu had proclaimed, the same boundaries "marked out by nature" invoked by Georges-Jacques Danton. From the architect of absolutism to the representatives of the National Convention, the idea had been a guiding principle of foreign policy and a central term in the definition of French unity. According to historian Albert Mathiez, writing in the 1920s, the Convention merely "cloaked in a red bonnet the old monarchical politics of natural frontiers"—even if the idea of natural frontiers had not always been explicitly invoked by those in power.²

The idea of France's natural frontiers still surfaces occasionally in textbook accounts of French expansion, but most historians of France today dismiss the "doctrine" of natural frontiers as too teleological a reading of France's history. In this, they owe an unacknowledged debt to French historian Gaston Zeller (1890–1960). A native of Clermont-Ferrand, Zeller taught for thirteen years at the University of Strasbourg, where in 1933 he succeeded Lucien Febvre in the chair of modern history.³ But, unlike Febvre, Zeller retained a passionate and unrelenting interest in "the history of events," writing extensively about France's eastern frontiers: the conquest and annexation of Metz, Lorraine, and Alsace during the sixteenth and seventeenth centuries. These concerns, combined with his own experience before and during World War II, profoundly influenced his attack on the "false idea" of natural frontiers.⁴

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¹ Albert Sorel, *Europe et la Révolution française*, Vol. 1: *Les Moeurs politiques et les traditions* (Paris, 1885), 244–337; 246.

² Albert Mathiez, *Histoire de la Révolution française*, 3 vols. (Paris, 1924), 2: 166.

³ Paul Vaucher, "Gaston Zeller," *Revue historique*, 225 (1961): 530–32; Georges Livet, "L'Institut et la chaire d'histoire moderne de la Faculté des lettres de Strasbourg de 1919 à 1955," *Bulletin de la Faculté des lettres de Strasbourg*, 36 (1957–58): 204–09.

⁴ Zeller's principal arguments are summed up in "La Monarchie d'Ancien Régime et les frontières naturelles," *Revue d'histoire moderne*, 8 (1933): 305–33; and "Histoire d'une idée fausse," *Revue de synthèse*, 11–12 (1936): 115–31. Further material is contained in *La France et l'Allemagne depuis dix siècles* (Paris, 1932), and his doctoral thesis, *La Réunion de Metz à la France (1552–1648)*, 2 vols. (Paris, 1926), 1: *passim*.

Zeller's pointed rebuttal of the "doctrine" focused almost exclusively on France's claim to the Rhine, an understandable fixation for a pacifist and patriot historian writing in a political climate of growing French and German militarism between the wars. Zeller believed that to write the history of the idea was to demystify the concept, to remove it from the realm of "journalism" and "ideology." It was a reaction shared by many others, among them Lucien Febvre, who published his own historical geography of the Rhine with Albert Demangeon in 1935.⁵ With typical erudition and impatience, Zeller demonstrated how irrelevant the "ideology" of natural frontiers was to French foreign policy objectives before 1792 and how a widespread invocation of the notion in the nineteenth and early twentieth centuries was tied more to popular textbook constructions of national identity, and to journalistic appeals to nationalism, than to historical verisimilitude.

Taking as a point of departure the life and work of Gaston Zeller and the rich and detailed evidence he collected, this essay reopens the case of France's natural frontiers. It examines the history of the idea since the seventeenth century in the double context of French foreign policy interests and the symbolic construction of French national identity. As a model for state building, the idea of natural frontiers sometimes provided the justification, sometimes the organizing principle, of French foreign policy. As a model of French identity, it formed part of a constitutive myth of the state. Natural frontiers appeared as one element within the shifting configuration of symbols and images of an ideal unity, a unity that drew alternately on the ideas of a shared language, a common history, and a bounded, delimited territory.⁶

Statesmen, diplomats, administrators, military officials, historians, and geographers all invoked the idea of natural frontiers as a defining feature of France's geography and history. In their hands, the idea frequently played a role within a legitimating discourse that served to rationalize French claims of territorial expansion and, occasionally, actually determined short-term foreign-policy objectives. Yet the functions played by the concept do not disclose its shifting fields of meaning over three centuries. The uses (and abuses) of the idea were framed by shifting conceptions of territory, history, and nature as these took shape within French state building since the seventeenth century.

AN OFT-CITED PASSAGE FROM Cardinal Richelieu's *Political Testament* once seemed sufficient to establish the centrality of the idea of natural frontiers within his overall

Lucien Febvre reviewed the thesis favorably, while criticizing Zeller's study of Metz for containing "too many facts and parasitical details"; *Revue d'histoire moderne*, 3 (1928): 44.

⁵ Lucien Febvre and Albert Demangeon, *Le Rhin: Problèmes d'histoire et d'économie* (Paris, 1935).

⁶ The distinction between "models of" and "models for" is adapted from Clifford Geertz, *The Interpretation of Cultures* (New York, 1973), 93–94, although I do not accept his idea of a "pre-established, non-symbolic system," which cultural models render meaningful. Recent studies of the symbolic construction of France include C. Beaune, *Naissance de la nation France* (Paris, 1986), and the contributions to the collective volumes edited by P. Nora, *Les Lieux de mémoire: La République* (Paris, 1984); *La Nation*, 3 vols. (Paris, 1986); and *Les France*, 3 vols. (Paris, forthcoming). Recent considerations of French space and national territory include Daniel Nordman, "Des limites d'Etat aux frontières nationales," in Nora, ed., *Les Lieux de mémoire, La Nation*, 2: 35–61; *Espace français: Vision et aménagement, XVI^e–XIX^e siècle*, catalogue of the exhibition at the Archives Nationales, September 1987–January 1988 (Paris, 1987); Joseph W. Konvitz, "The Nation-State, Paris, and Cartography in 18th and 19th Century France," *Journal of Historical Geography*, 16 (1990): 3–16; and Daniel Nordman and Jacques Revel, "La Formation de l'espace français," in *Histoire de la France*, Vol. 1: *L'Espace français*, Jacques Revel, ed. (Paris, 1989), 29–169.

foreign policy: "It was the goal of my ministry to restore to Gaul the limits that Nature has traced for her, to submit all the Gauls to a Gallic king, to combine Gaul with France, and everywhere the ancient Gaul had been, to restore the new one."⁷ Zeller dismissed the text because, in the tradition of Voltaire, he correctly doubted its authorship. Indeed, the apocryphal *Testamentum Christianum* (1643), from which the passage was drawn, was the work of Father Philippe Labbé, the Jesuit geographer and royal publicist. The text now attributed to Richelieu himself, "his thought although not his material realization," fails to mention Gaul or natural frontiers in the context of the Rhine, as do all of the cardinal's letters and policy directives. Zeller thus contended that the idea was at best mere propaganda done by some hanger-on at court, an isolated rhetorical flourish that played no part in the decisions of the French first minister.⁸

Yet equating contemporary France with ancient Gaul was not an isolated analogy in the France of Richelieu, nor was the invocation of the "limits that Nature has traced." History and geography formed part of a political culture that drew its language and images from the work of experts like Labbé—geographers, cartographers, and historians—most often in the service of the crown. These ideas of natural and historical frontiers constituted neither an "interest" nor an "ideology" but a belief that gave shape to an imagined national space, bounded and unified, in seventeenth-century France.

The Jesuit Jean François's *Science of Geography* (1652), like so much contemporary geographic discourse, stressed how mountains functioned "as very strong walls and ramparts between kingdoms, sufficient to stop the progress of a conqueror and the armies of an enemy. Such are the Pyrenees between France and Spain, and the Alps between France and Italy."⁹ Other geographers emphasized the role of mountains as "natural fortifications," among them Labbé, who published in 1646 his *Royal Geography Presented to Louis XIV*. Jesuit geographers and historians, supported and encouraged by the crown, diffused in their writings as in their classrooms the defensive image, elevated to a general principle, of natural frontiers.¹⁰ But they also brought forth the role of mountains and rivers in restraining and limiting the aggressive ambitions of a prince. An atlas at the time of Richelieu proclaimed that geographers study a kingdom's frontiers and teach them to the prince as "the limits of his ambition." It was not so much, as François Dainville has argued, "the idea of natural frontiers [*frontières naturelles*]" that was "anchored in the spirits" of the time but the image of limits or boundaries marked out by nature.¹¹

The linguistic distinction in French between boundaries (*limites*) and frontiers (*frontières*) in France dates from the late thirteenth century, when the French monarchy began to take account of the "frontier" of the kingdom as distinct from the jurisdictional boundaries of its suzerainty. The frontier was that which, etymologically and politically, "stood face to" an enemy. This military frontier,

⁷ Quoted in Zeller, "La Monarchie d'Ancien Régime," 311–12.

⁸ On questions of authorship and attribution, see Louis André, *Le Testament politique du Cardinal de Richelieu* (Paris, 1947), introduction; William F. Church, *Richelieu and Reason of State* (Princeton, N.J., 1972), 480–95 and *passim*; and Roland Mousnier, "Le Testament politique de Richelieu," *Revue historique*, 101 (1949): 55–71, quotation, 57.

⁹ Quoted in François Dainville, *La Géographie des humanistes* (Paris, 1940), 286.

¹⁰ See Dainville, *La Géographie des humanistes*, 185–87; and Jean Lecuir, "A la découverte de la France dans les abrégés d'histoire et de géographie des collèges jésuites du XVII^e siècle," in *La Découverte de la France au XVII^e siècle*, 9^{me} Colloque de Marseille organisé par le Centre Méridional de Rencontres sur le XVII^e siècle, 25–28 janvier 1979 (Paris, 1980), 298–317.

¹¹ Dainville, *La Géographie des humanistes*, 377; atlas quoted on 352.

implying bellicose expansion and a zonal defense, stood opposed to the linear boundary or line of demarcation—the *limites* of two jurisdictions or territories.¹²

The appearance of France's "frontiers" signified a movement, in image and reality, away from a set of traditional boundaries, commonly known from the tenth to the thirteenth centuries as the "four rivers." Medieval chroniclers consistently invoked the Saône, Rhône, Meuse, and Escault (Schelt) as the division of the kingdom of France and the Holy Roman Empire.¹³ To the medieval imagination, functioning without maps, the four rivers conveniently described the unity, and occasionally the political claims, of the French kingdom. Into the sixteenth century, according to an anonymous contemporary description of France, "common opinion" named the four rivers as the "limits of the Empire and the kingdom" of France. Learned opinion, relying on "ancient chronicles, histories, and annales of France," frequently concurred.¹⁴

Nonetheless, from the late thirteenth century, descriptions of the French kingdom as "closed" by the four rivers came to be challenged. It is true that "common opinion" showed a remarkable reluctance to abandon the idea, even into the twentieth century.¹⁵ But "learned opinion"—erudition in the service of the crown—frequently diverged. As cessions were made and conquests accumulated, the four rivers represented an increasingly inaccurate description of the kingdom's boundaries. The submission of Dauphiné to the French king in 1343 and the incorporation of Provence in 1481 gave the crown territories well to the east of the Rhône; along France's northern frontier, the Treaty of Madrid in 1526 had given the counties of Flanders and Artois to the Habsburg emperor, thus pushing back France's boundary far south from the Escault; and in 1601, Henri IV acquired from the kingdom of Savoy the provinces of Bresse, Bugey, Valromey, and the Pays de Gex, on the left bank of the Rhône.¹⁶

With these new acquisitions and changing frontiers, the crown needed new arguments to support its claims. Not nature but history became the legitimating discourse of foreign-policy decisions. In 1537, an anonymous *mémoire* argued that the Meuse river did not separate the kingdom of France and the empire, since the French kingdom extended "in several lands and seigneuries beyond the said river."

¹² Lucien Febvre, "Frontière, Evolution of a Word and a Concept," in *A New Kind of History: From the Writings of Febvre*, P. Burke, ed. (London, 1983), 208–11; and Daniel Nordman, "Frontière e confini in Francia: evoluzione dei termini e dei concetti," in C. Ossola, C. Raffestin, and M. Ricciardi, eds., *La frontiera da stato a nazione: il caso Piemonte* (Rome, 1987), 39–55.

¹³ Chroniclers and publicists throughout the Middle Ages named the four rivers as a gloss on the Treaty of Verdun in 843, that "territorial charter of Europe"; see Bernard Guenée, "Les Limites," in Michel François, ed., *La France et les Français* (Paris, 1972), 50–54; Henri Stein and Louis Le Grand, *La Frontière d'Argonne (843–1659): Procès de Claude la Vallée (1553–1561)* (Paris, 1905), 2–5; and Roger Dion, *Les Frontières de la France* (Paris, 1947), 71–85.

¹⁴ The anonymous description may be found in MS 472, fol. 10, Collection Dupuy, Bibliothèque Nationale (hereafter, BN); see also Bernard Guenée, "Des limites féodales aux frontières politiques," in Nora, ed., *Les Lieux de mémoire, La Nation*, 2: 26. In the fifteenth century, Gilles le Bouvier, Royal Herald of Charles VII, began his *Livre de la description des pays* with an account of how France was "closed" (*farmé*) by these rivers, and went on to describe other rivers that "close" or "separate" (*départ*) other kingdoms or countries described; see the text, Jules Theodore Ernest Hamy, ed. (Paris, 1908), 32–33. In the case of Argonne, however, the Biesme river was considered the boundary of France and the empire throughout the medieval period. The idea of the Meuse and the "four rivers" only made its appearance in the sixteenth century, as part of the renaissance of historical studies and within defense of new French claims; see Stein and Le Grand, *La Frontière d'Argonne*, 26–27.

¹⁵ Guenée, "Des limites féodales," 24.

¹⁶ For a summary of these and subsequent annexations, see Léon Mirot, *Manuel de géographie historique de la France*, 2 vols. (Paris, 1947–50); and Eugene Jarry, *Provinces et pays de France*, 4 vols. (Paris, 1950), vol. 1, *La Formation de l'unité française*.

Instead, the extension of the French kingdom "must be understood and explicated by ancient titles, or by the continuous possession of rightful heirs."¹⁷

The early modern state inherited a long-established royalist historiography that framed a specifically French national identity. In the sixteenth century, under the influence of Italian humanist philology and legal scholarship, and in the context of political upheaval during the Wars of Religion, French intellectuals construed a new image of France's *national* past.¹⁸ But the use and abuse of history within the monarchical projects of French expansion only came into its own during the seventeenth century, when Richelieu and the crown systematically organized research into the rights and claims of France to its newly "conquered provinces." Sometimes researched and written *ex post facto*, this work was frequently published, with the crown's privilege and blessing, for a wider reading public. Pierre Dupuy, Jacques de Cassan, Théodore and Denys Godefroy, and other "royal historians" developed claims that rested on the idea of "fundamental law" and the prohibition against alienating the royal domain, and on dynastic claims derived from sometimes-dubious past alliances and the customary laws of the jurisdiction in question. In the service of the crown, these tracts established French claims beyond its borders within the idiom of what Herbert Rowen has called "proprietary dynasticism."¹⁹

This discourse was appropriate, since the early modern French state was not yet, strictly speaking, territorial in nature. Its governing idiom was that of jurisdictions, including "appurtenances, dependencies, and annexes," to use the contemporary terms. Jurisdictional sovereignty in early modern France meant that the crown accumulated rights to specific domains—fiefs, bailiwicks, bishoprics, seigneuries, boroughs, and even villages. Alongside and overlapping these "feudal" forms of dominion were administrative circumscriptions created by the crown.²⁰ As seen from the periphery, the lesson was clear: the kingdom was not a coherent territorial entity consistently "bounded" in a linear sense. It is true that many of the medieval French crown's frontiers were boundaries, well-defined limits marked by boundary stones, rivers, trees, and sometimes trenches.²¹ But, despite the presence of delimited sectors of the boundary, most of France's borderlands in the early modern period were riddled with incoherent "provinces" made up of overlapping

¹⁷ MS 472, fol. 22, Dupuy Collection, BN.

¹⁸ On the medieval traditions of historical writing, see Beaune, *Naissance de la nation France*, *passim*; and Bernard Guenée, "Les Grandes Chroniques de France: Le Roman aux roys (1274–1518)," in Nora, ed., *Les Lieux de mémoire, La Nation*, 1: 189–214. On sixteenth-century historiography, see Donald R. Kelley, *Foundations of Modern Historical Scholarship: Language, Law, and History in the French Renaissance* (New York, 1970).

¹⁹ Herbert Rowen, *The King's State: Proprietary Dynasticism in Early Modern France* (New Brunswick, N.J., 1980); Church, *Richelieu and Reason of State*, 349–71; Zeller, "La Monarchie d'Ancien Régime," 316–18. On the monarchy's use of learned historical research in the later seventeenth century, see Joseph Klaitis, *Absolutism and Public Opinion: Printed Propaganda under Louis XIV* (Princeton, N.J., 1976).

²⁰ Peter Sahlins, *Boundaries: The Making of France and Spain in the Pyrenees* (Berkeley, Calif., 1989), 28–29, 54–59. An older literature treated the jurisdictional character of sovereignty as an essentially "feudal" formation; see, for example, Paul de Lapradelle, *La Frontière* (Paris, 1928), 35–50; Armand Brette, *Les Limites et divisions territoriales de la France en 1789* (Paris, 1907); and Gustave Dupont-Ferrier, "L'Incertitude des limites territoriales en France du XIII^e au XVI^e siècle," in *Compte rendu de l'Académie des Inscriptions et Belles-Lettres* (Paris, 1942), 62–67. For a reevaluation of the problem that emphasizes the coherence of judicial and administrative circumscriptions within territorial annexations, see Nelly Girard d'Albissin, "Propos sur la frontière," *Revue historique de droit français et étranger*, 47 (1969): 390–407; and, more recently, Paul Allières, *L'Invention du territoire* (Grenoble, 1980).

²¹ Guenée, "Des limites féodales," *passim*; Jean François Lemarignier, *Recherches sur l'hommage en marche et les frontières féodales* (Lille, 1945); and Paul Bonenfant, "A propos des limites médiévales," in *Hommage à Lucien Febvre: Eventail de l'histoire vivante*, 2 vols. (Paris, 1953), 1: 73–79.

and frequently disputed jurisdictions. An eighteenth-century description of Lorraine typifies the confused state of affairs throughout the early modern period. The province was "mixed, crossed, and filled with foreign territories and enclaves belonging with full sovereignty to the princes and states of Germany."²²

Gaston Zeller insisted that, within this sad state of affairs, the image of France's natural frontiers was "the offspring of cartography," and an illegitimate one at that. In fact, the mental image of a kingdom bounded by natural features long antedated the cartography of state. For the monarchy, which (beginning under Henri IV) increasingly brought cartographers and military engineers under its patronage, the image of natural frontiers was not terribly useful and less frequently represented on manuscript maps of the frontier provinces. But the stylized depiction of rivers and mountains within a growing commercial cartography provided a language that lent itself to the more general political project of building an idealized representation of the state.²³

In much commercial cartography of the mid-seventeenth century, the portrayal of mountains—depicted in perspective as if "viewed from horseback" and frequently simplified into chains—suggested the ideal of political divisions marked out by nature. Mountain ranges and sometimes rivers duplicated, if they did not determine, the shape of the dotted or dashed lines that distinguished both provinces and states.²⁴ Such was the case among many seventeenth-century map makers, including Nicolas Sanson (1600–1667), "the government's first official cartographer," and a highly successful publisher of commercial atlases.²⁵ Sanson's cartography of state was not devoid of scientific techniques or contributions to the history of map making. Yet, seduced by the idea of natural limits, and untroubled by a relative ignorance of topography in California as in Africa, he invented mountain ranges forming political boundaries where in fact there were none.²⁶ (See Figure 1.)

Such emphasis on mountains and rivers as political divisions was not confined to other continents; in 1627, a younger Sanson had published a map of "the Gauls" that won him Richelieu's favor and that portrayed the extension of ancient Gaul to the Rhine river. Many such maps were produced in the 1630s, and they were

²² "Observations concernant les limites du Royaume et les différen[t]s reglemen[t]s qu'on pourroit encore faire à cet égard," n.d., ca. 1775, *Limites* vol. 7, Archives du Ministère des Relations Extérieures (hereafter, AMRE).

²³ On cartography and statecraft in France, see Roland Mousnier, *The Institutions of France under the Absolute Monarchy, 1598–1789: Society and the State*, trans. Brian Pearce (1974; Chicago, 1979), 689–98; Louis Drapeyron, "L'Image de la France sous les derniers Valois (1525–1589) et les premiers Bourbons (1589–1682)," *Revue de géographie*, 24 (1889): 1–15; David Buisseret, "The Cartographic Definition of France's Eastern Boundary in the Early Seventeenth Century," *Imago Mundi*, 36 (1984): 72–80; Paul Solon, "Frontiers and Boundaries: French Cartography and the Limitation of Bourbon Ambition in Seventeenth-Century France," in *Proceedings of the Tenth Annual Meeting of the Western Society for French History* (Lawrence, Kan., 1984), 94–102; and, for the period after 1660, Joseph Konvitz, *Cartography in France, 1660–1848: Science, Engineering, and Statecraft* (Chicago, 1987), 1–31. The definitive descriptive bibliography of printed French atlases for the period is Mireille Pastoureau, *Les Atlas français, XVI–XVII^e siècle* (Paris, 1984).

²⁴ On the symbols of early modern cartography, see François Dainville, *Le Langage des géographes: Termes, signes, couleurs des cartes anciennes, 1500–1800* (Paris, 1964), esp. 271–83. For brief surveys of cartographic representations of mountains, see Jean Pierre Nardy, "Cartographies de la montagne, de l'édifice divin au bas-relief terrestre," in *Images de la Montagne: Catalogue et essais* (Paris, 1984), 77–79; and Konvitz, *Cartography in France*, 82–102.

²⁵ Konvitz, *Cartography in France*, 2; see also Mousnier, *Institutions*, 693–95; Mireille Pastoureau, "Les Atlas imprimés en France avant 1700," *Imago Mundi*, 32 (1980): 45–72.

²⁶ See the examples of natural frontiers on the many plates published by his son Guillaume and dedicated to Monseigneur Le Tellier, secretary of state; Nicolas Sanson, *L'Europe [L'Asie, l'Afrique, l'Amérique] en plusieurs cartes et en divers traittés de géographie et d'histoire* (Paris, 1683).

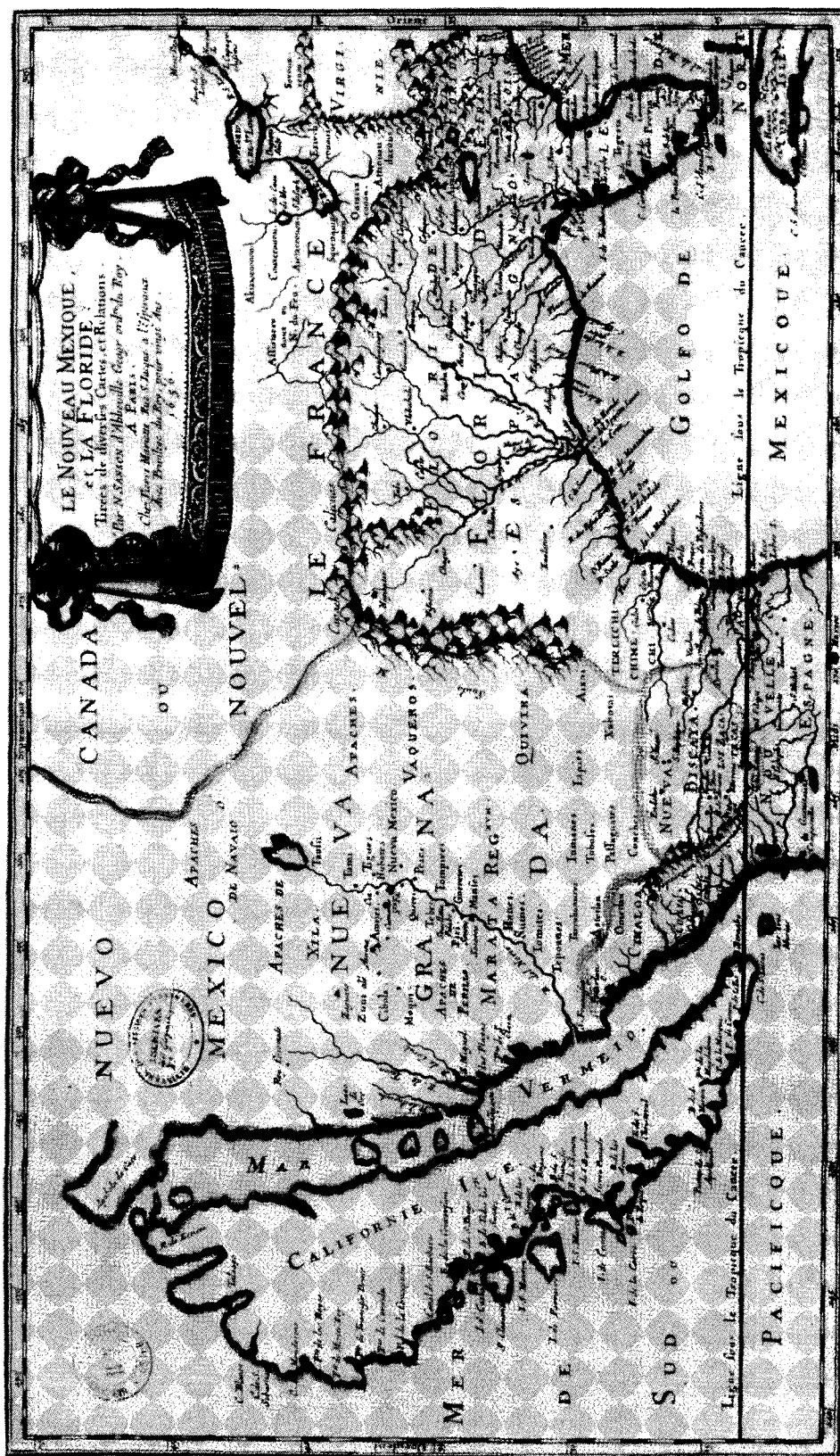


FIGURE 1: Nicolas Sanson, *La Nouveau Mexique et la Floride: Tirées de diverses cartes, et relations* [New Mexico and Florida] (1656), showing an imagined natural frontier of New France and Spanish Florida. Bibliothèque Nationale, Paris.

matched by the numerous published descriptions, authorized if not actively encouraged by Richelieu, that equated France and Gaul—in contrast to the Middle Ages, when “Gaul” and “Francia” remained distinct ideas.²⁷ (See Figure 2.)

As the politics of French expansion shifted to the northeast during the seventeenth century, and the image of the “four rivers” receded in the landscape of French political culture, French claims to restore the natural limits of Gaul came to the fore—and surfaced in Richelieu’s *Testament*—to help shape conceptions of a unified state.²⁸ But did the idea influence specific foreign policy objectives? The Pyrenean frontier of France and Spain, unmentioned by Zeller, is the most evident test case, since Article 42 of the Peace of the Pyrenees (1659) was unique in the seventeenth century for its explicit invocation of the “doctrine.” Giving to France the province of Roussillon in the eastern Pyrenees, Article 42 declared, “The Pyrenees Mountains which anciently separated the Gauls from the Spains, shall henceforth form the division of the two kingdoms.” Cardinal Mazarin, who negotiated the treaty, had added these words, “which I suppose will not be useless.” Indeed, the phrase was more than a mere rhetorical flourish and did not simply mask military and strategic interests. The idea of natural frontiers served to justify French expansion to the Pyrenees and created a new set of territorial claims, beyond those present in the evocation of provinces and jurisdictions ceded.²⁹ It further provided a reading public with an image of France’s acquisitions as bounded territorial entities; and, here again, Pierre Duval’s commemorative map of the Treaty of the Pyrenees depicted a nonexistent mountain range dividing France and Spain in the Cerdagne valley. (See Figure 3.)

To the north, the idea of restoring to France the “natural limits of Gaul”—and pushing toward the Rhine—made little sense: Flanders, Hainault, and the Habsburg Low Countries were less provinces to be acquired than frontiers to be made defensible, since the proximity of the Spanish armies threatened the security of Paris, less than 150 kilometers away.³⁰ To the east, however, in what took shape as the provinces of Alsace and Lorraine, the case was different. The idea of expanding to the Rhine, considered to be France’s natural and historical frontier, had a certain number of partisans in the entourage of royal power during the later sixteenth and seventeenth centuries. None of the spokesmen were particularly high-placed in the royal council; indeed, most advocates of the “program” of the Rhine frontier were natives of the Rhineland and based there—as Zeller, whose own life and early

²⁷ Zeller, *La Réunion de Metz*, 1: 52–55; and Lecuir, “A la découverte de la France,” 308–11. This identification of France with Gaul was further encouraged by the translations and diffusion of Julius Caesar’s *Commentaries* and other classical texts describing Gaul’s natural boundaries, the Atlantic, Rhine, Alps, and Pyrenees; see Dainville, *La Géographie des humanistes*, 346–54; and Church, *Richelieu and Reason of State*, 358.

²⁸ By the 1640s, the idea of the “four rivers” was historical memory. Louis Coulon argued in his treatise on *French Rivers* (1643) that “our fathers did not think themselves French if they did not drink from the Escault, Scarpe, Lys, Meuse, Moselle, and Rhine”; quoted in Dainville, *La Géographie des humanistes*, 348. The claim is significant in suggesting a degeneration of the “classical” medieval doctrine of four rivers and, at the same time, an expansion of French claims, most notably to the Rhine.

²⁹ Mazarin to Le Tellier, October 7, 1659, vol. 61, fol. 232, *Mémoires et Documents*, Espagne, AMRE; on the negotiations and provisions of the Treaty of the Pyrenees, see Sahllins, *Boundaries*, chap. 1. In the thirteenth century, the royal counselors of Philip the Fair had defined the Pyrenees as a natural watershed of France and Aragon, arguing that “where the waters go down towards Gascony is the kingdom of France, and where they go down to Spain or Catalonia, those are the kingdoms of Spain, according to Isidore and the ancient chronicles”; Phillippe Lauer, “Une Enquête au sujet de la frontière française dans le Val d’Aran sous Philippe le Bel,” *Comité des travaux historiques et scientifiques: Bulletin de la section de géographie*, 35 (1920): 28–29.

³⁰ The fortification of the northeastern frontier is the subject of Zeller’s monograph, *L’Organisation défensive des frontières du Nord et de l’Est au XVII^e siècle* (Paris, 1928).

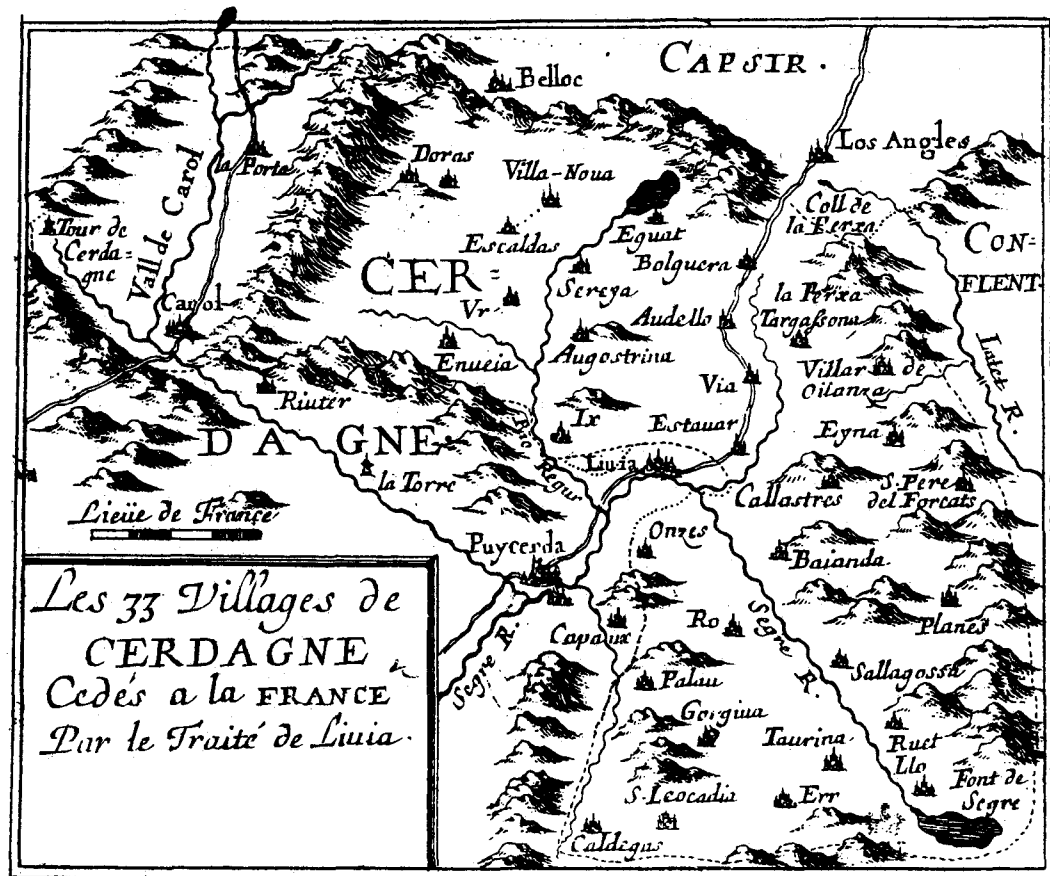


FIGURE 3: Pierre Duval, *Le 33 Villages de Cerdagne Cedés a la France Par le Traité de Llivia* [The 33 villages of Cerdagne ceded to France by the Treaty of Llivia], from his *Acquisitions de la France par la Paix . . .* [France's Acquisitions by Treaty] (Paris, 1679). Bibliothèque Nationale. The representation of natural frontiers is the typical fashion of seventeenth-century cartography: the mountains are drawn as seen "from horseback," the dotted lines are the same for provincial and for state boundaries, and the map shows a range of mountains dividing the Cerdanya in the southeast, when in fact there is none.

career took shape in the eastern borderland, was quick to point out. Thus Jean le Bon, born in Lorraine, physician to the cardinal of Guise, was author of (among other tracts) *Le Rhin au Roy* (1568), in which he coined the aphorism: "When Paris drinks from the Rhine, there Gaul will end" ("Quand Paris boira le Rhin/Toute la Gaule aura sa fin"). It is significant that the pamphlet was written after the 1552 conquests: throughout the sixteenth and seventeenth centuries, apologists from the provinces put forth claims to the Rhine frontier and the natural frontiers of Gaul to justify and occasionally to establish new claims in the Rhineland.³¹

³¹ Zeller, "La Monarchie d'Ancien Régime," 315–18. During the revolution as well, the "program" of the Rhine was first initiated by Rhinelanders loyal to the revolution (below, "32g"). The post facto character of the claim finds further evidence in the fact that, according to Zeller, the term *frontières naturelles* was first used by the royal historian Jacques-Auguste de Thou, writing in the first years of the seventeenth century, when he justified the conquest of Metz in 1552 by providing "the first proof, that of the natural frontiers of Gaul." The term became a widespread trope in historical descriptions of conquests: in 1688, Courtitz de Sandras claimed that Henri IV, at the beginning of the century, had sought "to extend the French kingdom to the shores of the Rhine, and in the south [*sic*] to the Alps"; quoted in Zeller, "La Monarchie d'Ancien Régime," 311, 314.

Zeller argued that such ideology never influenced Richelieu and his councilors, that Richelieu's foreign policy was guided by opportunism and grounded in dynastic principles—in "interests." True, in his private and public writings, Richelieu maintained that the French crown had to react to the universalist aspirations of the Habsburgs and to France's potential encirclement by their Spanish and German branches. The territorial implications of the policy were a series of "open doors to be able to enter into the neighboring states," as he put it. These frontiers thus served offensive aims as well, since Richelieu sought to assist local princes in the Rhineland and assert French presence in central Germany.³²

Not only was this foreign policy informed by the writings of "experts" who established the image of a unified, territorial state, it also gave natural frontiers an important role: the idea of natural frontiers was not, as historians as diverse as George Clark and Fernand Braudel have argued, simply the ideological mask of tactical interests.³³ Natural frontiers provided a concrete goal within Richelieu's overall policy around which specific diplomatic aspirations and military strategies were organized. Mountains and rivers were not limits that enclosed a French space, separating it from the other, but obstacles to be conquered—and passageways to be controlled—by establishing strongholds beyond them. As such, the idea of natural frontiers helped determine short and long-term policy decisions.

In the Pyrenees, this conquest of natural frontiers meant taking control of the towns and fortresses along the southern flank, as Mazarin sought to do in the 1650s, while waiting for Spain to begin peace negotiations. In the Alps, French policy throughout the sixteenth and seventeenth centuries was to maintain control over the towns and fortresses of Saluzzo and Pinerolo on the eastern side of the chain, as Zeller himself noted.³⁴ Richelieu's policy toward the Rhine involved establishing strongholds on its eastern bank: Philippsburg and Breisach, occupied in 1634 and 1639, were the principal fortified sites France sought to secure.³⁵ France emerged strengthened from the Treaty of Münster (1648), which forbade the emperor to build any fortresses on the right bank of the Rhine between Basel and Philippsburg, and gave to France Alsace (minus Strasbourg and Mulhouse) and its dependencies "on this side and the other of the Rhine."³⁶ It was an old medieval formula, but it served the military interests of the early modern state well. Natural frontiers were important to the French crown not as boundaries but as passages, and it was the plenipotentiaries of the empire who insisted on the Rhine as marking the separation of France from the empire.³⁷

Louis XIV continued and elaborated on Richelieu's Rhine policy, securing and building fortresses and strongholds on the eastern bank of the river: French armies occupied Freiburg in 1679, and the military engineer Sébastien Vauban constructed a series of fortresses (Fort-Louis, Kehl, and Juningue) on the right bank

³² Zeller, *L'Organisation défensive*, *passim*; see also H. Weber, "Richelieu et le Rhin," *Revue historique*, 239 (1968): 265–80; Richelieu is quoted in Nelly Girard d'Albissin, *Genèse de la frontière franco-belge: Les Variations des limites septentrionales de la France de 1659 à 1789* (Paris, 1970), 27–28.

³³ George Clark, *The Seventeenth Century* (New York, 1929), 148; and, more recently, Fernand Braudel, *The Identity of France*, Vol. 1: *History and Environment*, trans. Siân Reynolds (New York, 1988), 318–23.

³⁴ Sahlins, *Boundaries*, 42–43; Gaston Zeller, "Saluces, Pignerol, et Strasbourg: La Politique des frontières au temps de la prépondérance espagnole," *Revue historique*, 193 (1942): 97–110.

³⁵ Louis Battifol, "Richelieu et la question d'Alsace," *Revue historique*, 138 (1921): 161–200.

³⁶ Henri Vast, *Les Grandes traités de Louis XIV*, 3 vols. (Paris, 1894), 3: 131.

³⁷ Zeller, *L'Organisation défensive*, 71, 89; in his dismissal of the idea, Zeller returned frequently to the point that natural frontiers was more a creation of German than of French publicists: "La Monarchie d'Ancien Régime," *passim*.

during the 1680s.³⁸ But the French presence across the Rhine became a symbol of the hegemonic aspirations of Louis XIV, and, when faced with a defeat at the hands of the Augsburg League, France was forced at the Peace of Ryswick (1697) to restore most of its conquests and annexations since 1678, as well as all of its towns and dependencies on the east bank of the Rhine. Article 20 of what Vauban termed "this dishonorable peace" also ceded the fortress of Breisach to the Holy Roman emperor.³⁹ France had become "closed to the Germans," as stated by the motto inscribed on the medal struck in commemoration of Louis XIV's entry to Strasbourg in 1683; but, by the Treaty of Ryswick, France's capacity to intervene in Germany was also disabled.⁴⁰

The Treaty of Ryswick instituted France's Rhine frontier in the east, neither invoking the ancient frontier of Gaul (as had the Treaty of the Pyrenees forty years earlier) nor mentioning a general principle of states founded on natural divisions. But the Treaty of Utrecht in 1714, which ended the War of the Spanish Succession, named the "watershed of the Alps" as the separation of France and the kingdom of Savoy.⁴¹ For the first time, the principle of natural frontiers, stripped of its historical determination, was enshrined in a major peace treaty.

The idea of a unified and bounded territory, however, was not itself a novelty since it had been prepared for in the shifting politics of the frontier during the second half of Louis XIV's reign. In 1673, Vauban first gave expression to the politics of the frontier that France was to construct over the next thirty years. "The King ought to think a little about squaring his field. This confusion of friendly and enemy fortresses mixed together does not please me at all . . . Preach the squaring, not of the circle, but of the field; it is a good and beautiful thing to be able to hold one's accomplishment in both hands."⁴² This vision of a bounded and unified space ultimately took shape in Vauban's "iron frontier"—a double line of fortresses encircling France, begun after the Peace of Nijmegen in 1678 and completed by the turn of the eighteenth century. Replacing Richelieu's policies of "open doors," this new "politics of the barrier" came to serve similar functions: protecting France while enhancing its ability to intervene in German affairs. But the more apparently linear dimensions of Vauban's frontier suggest that the French crown was beginning to consider its territory a bounded unity and enclosed space.

Further evidence comes from the extensive conferences of French and Habsburg commissioners, who met after each of the peace treaties of Pyrenees (1659), Aix-la-Chapelle (1668), Nijmegen (1678), and Ryswick (1697). In each case, French commissioners entered into extensive negotiations with their counterparts in Spain and the Holy Roman Empire to settle jurisdictional disputes regarding France's northern frontier. While significantly reducing the number of enclaves and foreign jurisdictions on French territory, these treaties sometimes went so far as to

³⁸ Christopher Duffy, *The Fortress in the Age of Vauban and Frederick the Great, 1660–1789* (London, 1985), 19–20.

³⁹ The text is in Vast, *Grandes traités*, 3: 240; see Zeller, *L'Organisation défensive*, 103–06. To Vauban, the treaty was "dishonorable" even though he believed as a military engineer that France ought to give up its possessions beyond the Rhine; see Sorel, *L'Europe et la Révolution française*, 1: 312.

⁴⁰ Jacques Ancel, *Géographie des frontières* (Paris, 1938), 99; and G. Livet, "Louis XIV and the Germanies," in Ragnhild Hatton, ed., *Louis XIV and Europe* (London, 1976), 65.

⁴¹ Pierre Sopheau, "Les Variations de la frontière française des Alpes depuis le XVI^e siècle," *Annales de géographie*, 3 (1893–94): 194–96; Paul Guichonnet, ed., *Histoire et civilisations des Alpes*, 2 vols. (Toulouse, 1980), 1: 280–82.

⁴² Quoted in Zeller, *L'Organisation défensive*, 60; more generally, see chaps. 3 and 5, and Henri Chotard, *Louis XIV, Louvois, Vauban et les fortifications du nord de la France* (Paris, 1890).

demarcate a boundary line.⁴³ To the east, the so-called Chambers of Reunion of the 1680s annexed hundreds of dependencies and jurisdictions, helping to create a unified space that served both military and economic interests—even if France was forced to restore most of these in the Treaty of Ryswick (1697).⁴⁴

The later peace treaties of the reign of Louis XIV thus appear to give institutional shape to the ideal, first put forth by geographers and cartographers under Richelieu, of a state bounded by natural frontiers. By 1714, France had acquired much of its contemporary shape—minus the provinces of Lorraine and Savoy. At the same time, that shape had gained coherence as a unified territorial domain. The movement toward a territorial state, however, was far from complete: not only did the idea of territorial sovereignty remain undeveloped, in theory as in practice, but the political boundary in the north and east was largely undelimited. France's frontiers were riddled with enclaves, exclaves, overlapping and contested jurisdictions, and other administrative nightmares. The eighteenth-century state was to rationalize its administration and to rectify its limits, shifting the orientation of its policy from natural frontiers to natural boundaries.

IN GASTON ZELLER'S ACCOUNT, the problem of France's natural frontiers jumped from Richelieu to the revolution, bypassing the historical period in which the idea of a state bounded by natural, topographical features reached its most developed expression. This was unfortunate, for, isolated from its immediate eighteenth-century antecedents, the revolutionary doctrine of natural frontiers remains unintelligible. But, unlike the uses of the idea in revolutionary discourse, natural boundaries for the Old Regime monarchy played a less ideological function. Instead, they served to define both the ends and the means of French foreign and domestic policies. The idea underwent an important linguistic mutation: the occasional invocations of France's *frontières naturelles* were replaced by more insistent claims to France's *limites naturelles*. The movement from natural frontiers to natural boundaries was grounded in a double transformation of society and the state: the enlightened emphasis on nature stripped mountains and rivers of their historical content, while the attempts to reform a politically weak French state found practical uses for the ideas of natural boundaries.

Eighteenth-century intellectuals inherited from the geographical discourse of classical humanism and seventeenth-century political culture the notion that France ought to have as its boundaries the same mountains and rivers that had limited Gaul.⁴⁵ More widespread, particularly among the *philosophes*, was the idea that mountains and rivers as such, devoid of any historical determination, formed the limits of polities. Used widely in descriptions of European nations, this idea could be found on both sides of an intellectual debate. In 1748, Montesquieu's geograph-

⁴³ Girard d'Albissin, *Genèse de la frontière franco-belge*, *passim*.

⁴⁴ On the Chambers of Reunion, see Louis André, *Louis XIV et l'Europe* (Paris, 1950), 187–215; on Vauban and the economic and military rationales for the development of a unified (and centralized) space, see Pierre Dockes, *L'Espace dans la pensée économique du XVI^e au XVIII^e siècle* (Paris, 1969), 158–78.

⁴⁵ Learned historians like Dom Martin Bouquet continued to remind their readers, "Our Gaul, which is Gaul properly speaking . . . was contained between the Ocean, the Mediterranean, and the Alps, and extended from the Pyrenees mountains to the edge of the Rhine," while others wrote of France's rights to all "its previously dependent provinces, following the extension of ancient Gaul"; see Sorel, *L'Europe et la Révolution française*, 1: 319–20; see also Daniel Nordman, "L'Idée de frontière fluviale en France au XVIII^e siècle: Discours géographique et souveraineté de l'Etat," in *Frontières et contacts de civilisations*, Colloque Universitaire Franco-Suisse (Neuchâtel, 1979), 77–80.

ical determinism was commensurate with his identification of a state's "natural boundaries" (*limites naturelles*), which were both its right and a constraint on its expansion. David Hume, arguing instead for the "moral" determination of national character, still invoked the image of a natural boundary:

the same national character commonly follows the authority of government to a precise boundary; and upon crossing a river or passing a mountain, one finds a new set of manners, with a new government. The Languedocians and Gascons are the gayest people in France; but whenever you pass the Pyrenees, you are among Spaniards. Is it conceivable, that the qualities of the air should change exactly with the limits of an empire?

It was Rousseau, discussing the proper size of the state, who most emphatically underlined the natural limits of political nations: "The lie of the mountains, seas, and rivers [in Europe], which serve as boundaries of the various nations which people it, seems to have fixed forever their number and size. We may fairly say that the political order of the Continent is in some sense the work of nature."⁴⁶

Natural boundaries were the focus of much eighteenth-century geographical writing and teaching as well, a discourse that continued to serve the interests of the crown. Philippe Buache (1700–1773), royal geographer, member of the Academy of Sciences, and tutor of the royal children (the future Louis XVI, Louis XVIII, and Charles X), focused his talents on the cartographic and linguistic representation of mountains and rivers, emphasizing the linear character of mountain chains that necessarily divided watersheds. His nephew and disciple, Buache de la Neuville (1741–1825), who worked as a practicing and teaching *géographe royal*, went one step further, frequently emphasizing (both in writing and in his maps) a vision of politics divided by natural boundaries.

The natural division of the surface of the earth by mountains and rivers is one of the geographical considerations which ought to be developed . . . for it can contribute to the happiness of future nations, which will assure and defend more effectively the boundaries of their possessions by adopting constant and unchanging boundaries established by nature.⁴⁷

Within the national map survey begun by the monarchy in 1660, the natural limits of French territory remained part of the language of cartography. In the map produced by Jacques Cassini (1677–1756) when he completed an important step in France's geodetic survey in 1720—the triangulation of the Paris meridian begun in 1680—the natural boundaries of continuous mountain ranges in the south and east continued to inform his conception of French space.⁴⁸ (See Figure 4.)

The enlightened, de-historicized reinterpretation of natural boundaries coincided with a shift in the policies of state building in later eighteenth-century France. The era of annexations was over: the Treaty of Vienna in 1738 gave the province

⁴⁶ David Hume, "Of National Characters" [1748], in Peter Gay, ed., *The Enlightenment: A Comprehensive Anthology* (New York, 1973), 530; Montesquieu, *The Spirit of the Laws* [1748], D. W. Carrithers, ed. (Berkeley, Calif., 1977), bk. 14, *passim*; and 10: 9; Jean-Jacques Rousseau, *Extrait du projet de paix perpétuelle de l'Abbé de Saint Pierre* [1756], in C. E. Vaughn, ed., *The Political Writings of Jean Jacques Rousseau* (Cambridge, 1915), 1: 370.

⁴⁷ Daniel Nordman, "Buache de la Neuville et la 'frontière' des Pyrénées," in *Images de la montagne*, 107. On the Buache dynasty, see Louis Drapeyron, "Les Deux Buache ou l'origine de l'enseignement géographique par versants et par bassins," *Revue de géographie*, 22 (1887): 6–16; Drapeyron, "L'Éducation géographique de trois princes français au XVIII^e siècle, le duc de Berry et les comtes de Provence et d'Artois (Louis XVI, Louis XVIII, Charles X)," *Revue de géographie*, 22 (1887): 241–56; and Numa Broc, "Un Géographe dans son siècle: Philippe Buache (1700–1783)," *Dix-huitième siècle*, 3 (1971): 223–35.

⁴⁸ On the national map survey and the Cassini dynasty between 1660 and 1793, see Konvitz, *Cartography in France*, 1–31.

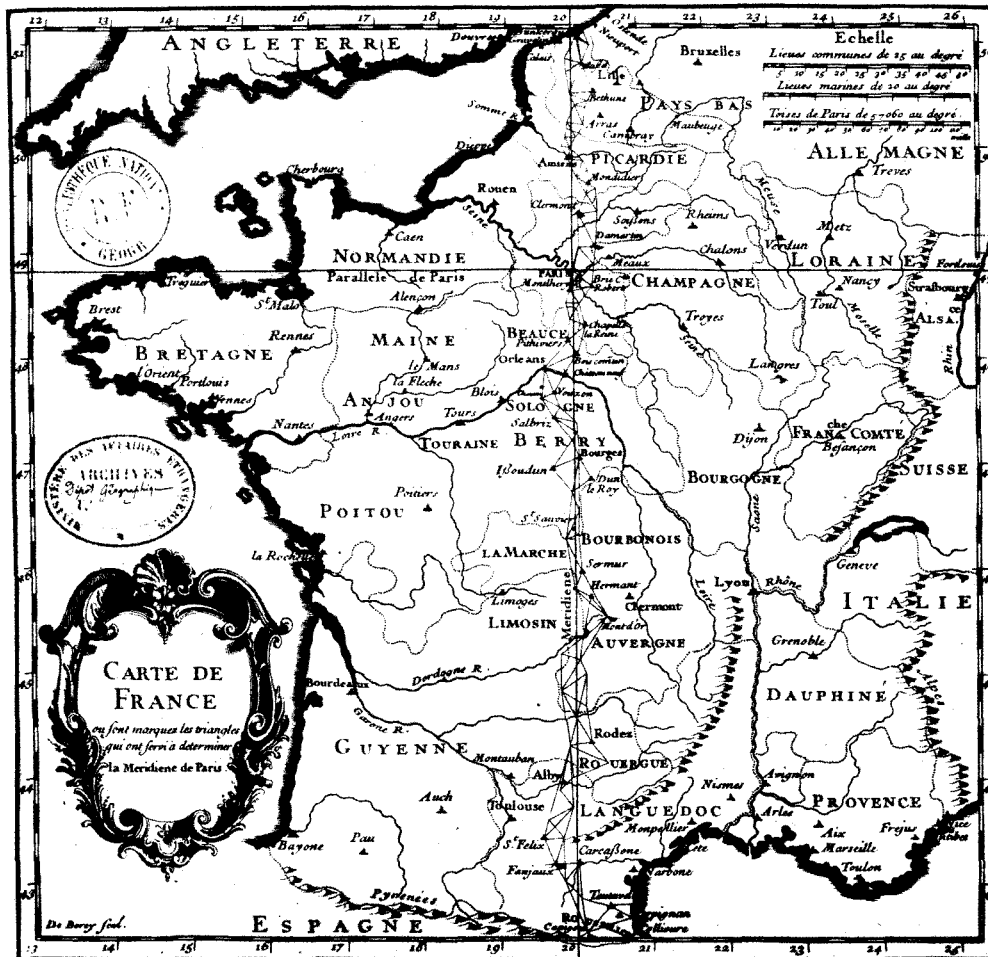


FIGURE 4: The Cassini map of the Paris Meridian (1720), showing its recently completed triangulation along with France's natural limits in the south and east. Bibliothèque Nationale.

of Lorraine to King Stanislas Leszczynski of Poland, and it passed to France on his death in 1766. Lorraine and Corsica (the latter bought by Louis XV in 1768) were the last of France's acquisitions under the Old Regime. As the marquis d'Argenson, foreign minister under Louis XV, wrote in his memoirs published in 1765, "This is no longer a time of conquests. France must be satisfied with its greatness and extension. It is time to start governing, after spending so much time acquiring what to govern." Indeed, France embarked on, and was to continue until the revolution, an ultimately unsuccessful administrative modernization—a bureaucratic reform program with important consequences in French foreign policy.⁴⁹

Many of the proposed administrative reforms in France involved creating a rational administration that could replace the existing chaotic and inefficient bureaucracy. Part of the problem lay in the absence of the "province" as a juridical

⁴⁹ The marquis d'Argenson is quoted in Sorel, *L'Europe et la Révolution française*, 1: 313; on administrative reform and bureaucratic modernization in eighteenth-century France, see John F. Boshier, *The Single Duty Project: A Study of the Movement for a French Customs Union in the Eighteenth Century* (London, 1964); and Boshier, *French Finances, 1770–1790* (Cambridge, 1977).

and administrative entity. In territorial terms, Frenchmen divided the kingdom into anywhere from thirteen to ninety-one "provinces": for the state, the province was a purely jurisdictional notion. Thus the Old Regime monarchy was divided into as many "provinces" as there were "political governments," "regimes," and "powers," to use the contemporary terms.⁵⁰ Enlightened bureaucrats and interested geographers proposed a juridical status for the "province" that would make it the exclusive framework for the different jurisdictions of the state. But their projects languished, and, at the end of the Old Regime, the kingdom of France had neither a coherent territorial administration nor, as Armand Brette and others have long pointed out, precisely defined limits.⁵¹

Yet, if the Old Regime French state failed to reform its administrative structure, it undertook in the second half of the eighteenth century to delimit systematically and demarcate with boundary stones its territorial boundary line with its neighbors. The acquisition of an important map collection of all of France's frontiers was essential to this task.⁵² More important, by 1775, the ministry of foreign affairs had gained jurisdiction over boundary matters from the war ministry and established its own topographical bureau for the demarcation of limits, creating permanent "commissioners" to negotiate "treaties of limits" with France's neighbors.⁵³ During the 1770s and 1780s, the French government signed more than two dozen "treaties of limits" with the neighboring polities making up the Holy Roman Empire, the Swiss cantons, the kingdom of Savoy, and Spain.⁵⁴ (See Table 1.) This attempted "rationalization" and "purification" of the political boundary formed an essential if frequently overlooked dimension of French policy in the later eighteenth century.

The goal of the state was primarily a domestic one: to create an enclosed, unified space in order to assure the efficient administration of the realm. A royal engineer at Besançon, the chevalier de Bonneval, wrote in a *mémoire* of 1745 that the idea behind delimiting France's boundaries was to "purge the kingdom of foreign enclaves" and to "close the state as far as the nature of the district permits." By ridding itself of enclaves and exclaves, the state sought to repress military desertion and fiscal fraud and to create a unified economic space.⁵⁵ Less abstractly, the point was to "determine the boundaries in a manner most clear and most evident for the respective subjects, and in the most permanent way possible, so as to destroy all

⁵⁰ Sahllins, *Boundaries*, chaps. 2 and 5. In 1790, the constitution committee reported that "the kingdom is divided into as many different divisions as there are diverse kinds of regimes and powers: into *diocèses* as concerns ecclesiastical affairs; into *gouvernements* as concerns the military; into *généralités* as concerns administrative matters; and into *bailliages* as concerns the judiciary"; quoted in Gustave Dupont-Ferrier, "Sur l'emploi du mot 'province,' notamment dans le langage administratif de l'ancienne France," *Revue historique*, 160 (1929): 262.

⁵¹ Armand Brette, *Les Limites et les divisions territoriales de la France en 1789* (Paris, 1907); Numa Broc, *La Géographie des philosophes* (Paris, 1978), 461; Louis Trenard, "Perception et délimitation de l'espace français au XVIII^e siècle," *L'Information historique*, 47 (1985): 124–25.

⁵² Konvitz, *Cartography in France*, 33–41.

⁵³ "Conservateurs de limites," including an ordinance of January 31, 1773, *Limites* vol. 7, AMRE, creating an inspector "charged with operation and affairs regarding exchanges and concessions of territory, from sovereign to sovereign, and with regulations of limits of the states and possessions of His Majesty with those of neighboring states." On the shift of jurisdiction under the comte de Vergennes, who held both portfolios at the time, see "Mémoire sur les affaires des limites (écrit vers 1770)," 4.3.1, no. 24, Archive de l'Inspection du Génie, Château de Vincennes (hereafter, AIG).

⁵⁴ Jean-François Noël, "Les Problèmes de frontières entre la France et l'Empire dans la seconde moitié du XVIII^e siècle," in *Revue historique*, 235 (1966): 333–46.

⁵⁵ "Mémoire sur l'opération de l'établissement des limites du royaume," April 22, 1745, *Limites* vol. 7, no. 4, AMRE; see Allières, *L'Invention du territoire*, 62–77.

TABLE 1
Principal Delimitation Treaties, 1738–1785

	<i>Date</i>	<i>Source</i>
Austria (Luxembourg)	March 22, 1738	Koch 1: 335–36
Republic of Geneva	August 15, 1749	Limites 376, AMRE
	December 26, 1752 (D)	
	December 29, 1763 (D)	
Principality of Salm	December 21, 1751	Limites 150, AMRE
Duchy of Würtemberg	February 4, 1752 (P)	Koch 1: 493–96
	May 21, 1786	Märtens 2: 652–64
Kingdom of Savoy	March 24, 1760	Limites 344, AMRE
	April 15, 1761 (D)	4.3.1, no. 14, AIG
Prussia (Neuchâtel and Valengrin)	September 28, 1765	Koch 2: 208–22
Austria (Low Countries)	May 16, 1769	Märtens 1: 265–80
	November 18, 1779	Märtens 2: 56–67
Bishopric of Liège	October 9, 1767 (P)	Koch 2: 265–68
	May 24, 1772	Märtens 1: 292–309
	December 9, 1773 (D)	Märtens 2: 499–502
	June 11, 1778	Koch 2: 459–62
Electorate of Treves	October 29, 1773 (P)	Koch 2: 321–33
	July 1, 1778	Märtens 4: 181–89
Principality of Nassau-Saarbrücken	June 9, 1760 (P)	Koch 2: 141–61
	February 15, 1766	Märtens 1: 154–79
	October 26, 1770	Koch 2: 289–99
Canton of Berne	November 15, 1774 (D)	Koch 2: 352–95
Principality of Nassau-Weilburg	January 24, 1776	Märtens 1: 552–71
County of Leyen	September 21, 1781	Märtens 2: 138–67
Duchy of Deux Ponts	May 10, 1766	CP Deux-Ponts suppl.
	April 3, 1783	5, 122, 125, AMRE
	November 15, 1786	
Bishopric of Basel	December 7, 1779	
	June 20, 1785	Koch 2: 477–91
Spain	August 27, 1785	Koch 2: 477–91

Abbreviations:

(P): Preliminary Accord

(D): Delimitation Agreement

AMRE: Archive du Ministère des Relations Extérieures (Paris).

CP: Correspondence Politique

AIG: Archive de l'Inspection de Génie (Château de Vincennes).

SOURCES: G. F. de Märtens, *Recueil des principaux traités d'alliance, de paix, de trêve, de neutralité, de commerce, de limites, d'échange, etc., conclus par les puissances de l'Europe tant entre elles qu'avec les puissances et Etats dans d'autres parties du monde depuis 1761 jusqu'à présent*, 4 vols. (Göttingen, 1791–1801).

C. Koch, *Table des traités entre la France et les puissances étrangères depuis la paix de Westphalie jusqu'à nos jours*, 2 vols. (Basel, 1802).

objects of dispute among frontier inhabitants.”⁵⁶ Indeed, many of the delimitation treaties were drawn up in attempts to resolve specific disputes—not just between customs guards or soldiers of neighboring states but among inhabitants on opposite sides of the boundary. Pastures, waters, and usufruct rights on opposing mountain watersheds and riverbeds themselves were the objects of local quarrels: the state believed that its failure to define its exact territorial extension led to an escalation of local conflicts. In attempting to repress “territorial violations,” the state sought to delimit its territorial extension. Emphasizing the territorial over the jurisdictional, the French state undertook to bound and enclose a territory by defining its limits “marked by nature.”⁵⁷

“It is good to take streams, rivers, watersheds, or finally straight lines, when none of these [others] exist . . . as the boundaries of territories,” reads a *mémoire* sent to the foreign minister in 1772.⁵⁸ And it was commonplace to name natural boundaries, both specifically and generally, in the prefaces and major clauses of the “treaties of limits” during the 1770s and 1780s. The delimitation treaty between the kingdoms of France and Savoy on March 24, 1760, ordered “an exact, general, and definitive fixing of the boundaries which must hereafter separate their respective states and countries, which fixing, as far as the territory may permit, will be established according to riverbeds or watersheds, and assisted by a rectification or exchange of different enclaves.”⁵⁹ The 1785 accord that delimited the French-Spanish boundary in the western Pyrenees insisted on a “dividing line which separates and divides all the lands of the two powers, the property of the valleys, and the sovereignty of the two kings,” and it reinterpreted the Treaty of the Pyrenees in terms of an enlightened conception of nature.⁶⁰

But it was rivers that attracted the attention of statesmen and commissioners, especially those along France’s northern and eastern frontiers.⁶¹ Article 8 of the treaty between the king of France and the duke of Würtemberg (May 21, 1786) took the Doubs river as the limit of the two dominions, and further articles specified smaller streams as boundaries. The Doubs was also named the “fixed and natural limit” of France and the principality of Basel in a treaty of June 1780. The treaty between France and the electorate of Treves (July 1, 1778) named the Saar river as “the natural boundary of the two dominions.”⁶²

There is a double paradox in the French fascination with natural boundaries as

⁵⁶ “Mémoire sur les frontières du Royaume entre l’Océan et le Rhin, et sur les réglemens de limites qu’il serait avantageux d’y faire,” by M. de Grandpré, May 26, 1772, *Limites* vol. 7, AMRE.

⁵⁷ Sahlins, *Boundaries*, 92–103.

⁵⁸ “Mémoire sur les frontières du Royaume,” *Limites* vol. 7, AMRE.

⁵⁹ Treaty of March 24, 1760, *Limites* vol. 344, no. 110, AMRE.

⁶⁰ *Limites* vol. 459, nos. 91–92, AMRE; “Commission de Limites, Espagne,” *Limites* vol. 463, AMRE; and “Reflexions sommaires sur la fixation des limites,” MR 1084, no. 75, Ministère de la Guerre, Archives de l’Armée de la Terre (hereafter, MG AAT). On the reinterpretation of the Treaty of the Pyrenees, see, for example, “Première mémoire sur la frontière du Roussillon,” September 24, 1777, *Limites* 439, no. 91, AMRE.

⁶¹ Nordman, “L’Idée de frontière fluviale,” 84–88; Girard d’Albissin, “Propos sur la frontière,” 401–05.

⁶² Sources for these treaties appear in Table 1. On Lorraine, where France sought to establish a definitive regulation of the boundary such that “a portion of the Saar, Blise, Horne, and Queiche rivers serve as the limits of royal sovereignty,” see “Mémoire concernant les limites du pais de la Sarre, Lorraine Allemande, Basse-Alsace,” 4.3.1, no. 10, AIG. As Nordman has pointed out, the exact formula concerning the extension of sovereignty to rivers varied considerably in the eighteenth-century treaties: some named rivers to be held “in common” (the Saar); in some cases, both banks were held by a single sovereign (the Doubs, over which the prince-bishop of Basel maintained dominion); and, in some instances, the waterway was divided in half (the 1760 treaty concerning the Rhône, Estéron, and Var); Nordman, “L’Idée de frontière fluviale,” 87.

TABLE 2
Ratifications by the Diet of the Holy Roman Empire of Treaties with France

Nassau-Saarbrücken, 1766	February 1, 1768	Märtens 3: 241–42
Liège, 1773	April 22/May 11, 1774	Märtens 1: 502–06
Nassau-Weilburg, 1776	July 11, 1785	Märtens 2: 580–82
Basel, 1780	July 11, 1785	Märtens 2: 587–88
La Leyen, 1781	July 11, 1785	Märtens 2: 590–92

SOURCES: G. F. de Märtens, *Recueil des principaux traités d'alliance, de paix, de trêve, de neutralité, de commerce, de limites, d'échange, etc., conclus par les puissances de l'Europe tant entre elles qu'avec les puissances et Etats dans d'autres parties du monde depuis 1761 jusqu'à présent*, 4 vols. (Göttingen, 1791–1801).

the tools of state building. For one, the policy of establishing these boundaries appeared less as a function of France's reputation for international strength than as a sign of its weakness. As the geo-political arena of competition shifted to Eastern Europe, where France was increasingly excluded, and to the New World, where France was defeated, France's political boundaries became the sites of micro-political contention between the French court and its undistinguished neighbors. Ideally, the French court would have liked to deal directly with the Imperial administration, as it had after the Treaty of Vienna (1738) ordered France and the empire to regulate the boundaries of Lorraine. Commissioners met over three years, but, when Charles VI died in 1741, negotiations broke off. France hesitated to take up talks with the principalities, fearing that "the private interest would carry the public."⁶³ While the imperial court at Vienna ratified several of the "treaties of limits" signed between France and its German neighbors (see Table 2), the actual negotiations took place among the German princes and bishops. Unlike the seventeenth-century treaties, in which (the chevalier de Bonneval noted) "a victorious power dictated terms to a vanquished one," the eighteenth-century treaties of limits involved the negotiation of two formally equal powers.⁶⁴ In formally recognizing the smaller states as equals, however, the French court was constantly forced to entertain the sometimes fantastical claims of Lilliputian states. Thus France's natural boundaries were imposed on it by diminutive bishoprics and petty principalities.

The second paradox is more obvious. A political boundary marked by a natural topographical feature was much less durable, and infinitely more contested, than one drawn arbitrarily without regard to the landscape but following extant jurisdictional divisions. Such was the case even for mountains. The French-Spanish commissioners of 1784–1785 sought a political division that did not deviate from the watershed of the Pyrenees, although, in doing so, they defined a boundary of France and Spain as impractical as it was abstract. Village and valley communities on either side of the Pyrenees maintained rights of usufruct and property on opposing versants; they depended on—and frequently fought over—reciprocal rights to pasture, wood, and passage across the territory of the other. Eliminating

⁶³ "Mémoire concernant les limites," 4.3.1, no. 10, AIG. Despite such virtuous claims, the *mémoire* suggested that the French government might buy off the electors of Palatinate and the duc of Deux-Ponts, "who need a lot of money."

⁶⁴ Bonneval, in *Limites* vol. 7, no. 4, AMRE. Of course, France could still dictate terms. When the count of La Leyen refused an exchange offered by the marshal Belle-Isle, the royal courts of Lorraine ordered the seizure of the count's lands under French control, and he acquiesced; see Noël, "Les Problèmes de frontières," 339.

these in favor of a natural boundary meant denying the interests of local subjects, who, as a result, only increased their disputes, both among themselves and with their respective states.⁶⁵

The case of rivers suggests the problematic nature of natural boundaries. For example, seigneuries, parishes, and towns were frequently established on both sides of a river: the historical geography of settlement is proof that rivers tended to join more than they divided.⁶⁶ Moreover, rivers were likely to overflow their banks with some regularity. The Raour stream, for instance, divided France and Spain (though not the parish and property of Hix) in the Cerdanya. As a youthful river, the Raour was constantly subjected to flooding, just as it was manipulated by riverfront proprietors who built dikes and barrages to redirect the stream bed and protect their properties.⁶⁷ The case of the Rhine was more striking still. A military engineer from Strasbourg noted in 1814,

Everybody agrees that all boundaries should be as fixed and as invariable as possible; yet what is more variable than the middle of the Rhine, that is to say, the navigable part of the river? The Rhine changes its course every year, sometimes two or three times. With the floods, an island or a commune, which in the spring was French, is German the following winter, then becomes French again in two or three years, and by dams or dikes, the riverfront inhabitants and sometimes the contiguous states bring back an island to their respective banks. These islands, without stable and recognized masters, facilitate disorders of every kind.⁶⁸

Rivers were intrinsically subject to disputes, especially when opposing proprietors were not under the same political jurisdiction. Yet the "doctrine" was so firmly entrenched in French foreign policy that, even when faced with the obvious evidence that it did not work, the foreign ministry continued to insist on it—thus giving employment to dozens of commissioners and engineers charged with periodically "rectifying" France's natural boundaries.⁶⁹

Why should this have been so? In part, it was because military interests had captured natural boundaries as their own. Although the foreign ministry took over the formal jurisdiction of boundary matters from the war ministry in 1772, the military establishment continued to exercise an important influence in the formulation of French policy. Military engineers and generals fully recognized that, in a state of war, the superior army "would always be master of the most advantageous positions, wherever they are situated."⁷⁰ But, while military interests in the eighteenth century sometimes echoed seventeenth-century claims to control an opposing watershed or riverbank (as along the Rhine), the dominant position was to adopt mountain crests and middles of rivers as boundaries. Military thought was

⁶⁵ Sahlins, *Boundaries*, 98–99; Carlos Fernandez de Casadevanti Romani, *La Frontera hispano-francesa y las relaciones de vecindad (Especial referencia al sector fronterizo del país vasco)* (San Sebastian, 1986), *passim*; and Christian Desplat, "Le Parlement de Navarre et la frontière franco-navarraise à l'extrême fin du XVIII^e siècle," in Jean-François Nail, *et al.*, *Lies et passerelles dans les Pyrénées* (Tarbes, 1986), 109–20.

⁶⁶ Zeller, *La Réunion de Metz*, 1: 28–29; Girard d'Albissin, "Propos sur la frontière," 401–03.

⁶⁷ Comte d'Argenson to Intendant of Roussillon, December 13, 1750, *Limites* vol. 459, no. 57, AMRE; "Mémoire sur le redressement du Raour," n.d., ca. 1750, *ibid.*, no. 50; and maps and *mémoires* concerning the 1750 rectification, 4.3.2, AIG.

⁶⁸ "Observations sur les limites françaises sur le Rhin et vers le Palatinat," by the Corps du Génie, Strasbourg, May 1814, 4.3.1, no. 28, AIG.

⁶⁹ From the 1730s to the early 1790s, the Noblat family received commissions from the ministry of foreign affairs to "rectify" and "realign" the riverbeds forming the frontiers with the Swiss cantons and the Holy Roman Empire; on Switzerland, see *Limites* vol. 361, *passim*; and "Conservateurs de Limites," *Limites* vol. 7, AMRE.

⁷⁰ "Mémoire sur les frontières entre l'Océan et le Rhin," May 26, 1772, *Limites* vol. 7, AMRE.

also influenced by an enlightened conception of natural boundaries, stripped of their historical determinations.⁷¹

Focusing on the idea and practice of natural boundaries, the Old Regime state implanted a largely linear, territorial boundary: by 1789, there were few enclaves left, and the final delimitation treaties of the early nineteenth century were, except for the Pyrenees, simply corrective.⁷² Both the ideal and reality of France's natural boundaries were largely in place by the time of the revolution, and it was on this foundation that the revolutionary governments built an ideology of French expansion.

THE CLAIMS OF SOREL, MATHIEZ, AND OTHERS—that the French revolutionaries merely continued the Old Regime policy of moving toward France's natural frontiers—are thus both true and false. It is true that the revolutionary state drew on an official practice and philosophical ideal of natural boundaries taking shape during the eighteenth century. But the particular political and ideological program of the Rhine frontier was a revolutionary invention—a product of the revolutionary process.

The first two years of the revolution saw few invocations of the doctrine. Buache de la Neuville wrote to the minister of foreign affairs in July 1791, urging the fixing of France's limits according to "the natural division of the Globe formed at its origin by the Creator," but neither the court nor the National Assembly was much interested in demarcating the political boundaries of France.⁷³ Such a policy had little resonance within the universalizing tendencies of the early revolution, with its focus on the abstract universals of the Rights of Man and Citizen. The famous "no conquests" formula of May 22, 1790, inscribed in the Constitution of 1791, declared, "The French nation renounces the undertaking of any war with a view toward conquests, and will never use its forces against the liberty of any people." Revolutionaries threw their moral and political support to Patriot parties across Europe. The boundaries of France—and of the revolution—were not marked by nature or by history but, as Johann Wolfgang Goethe suggested in a famous watercolor of 1790, by the invisible principle of liberty.⁷⁴ (See Figure 5.)

National boundaries, and, with them, the doctrine of natural boundaries, only became relevant as France regained its hegemonic aspirations in the revolutionary wars against the "despots" of Europe.⁷⁵ France's declaration of war on Prussia and Austria in the spring of 1792 was followed by a series of defeats during the summer that led to the fall of the monarchy. Then, after the victories of the Republic at

⁷¹ Noël, "Les Problèmes de frontières," 338–39; Nordman, "L'Idée de frontière fluviale," 85–87; Girard d'Albissin, "Propos sur la frontière," 404–05. Concerning the rectification of the Pyrenean frontier, a military engineer argued that the line occasionally "left the crest" because it had been established on the basis of "such vague antiquities as the limits of Narbonese Gaul" instead of coinciding with the watershed; see "Mémoire sur les limites de la frontière en Roussillon," 1775, 4.3.2, no. 2, AIG; and *Mémoires on the Roussillon frontier*, 1777, MR 1084, nos. 41 and 43, MG AAT.

⁷² Girard d'Albissin, *Genèse*, 299–363; compare Charles Rousseau, *Les Frontières de la France* (Paris, 1957).

⁷³ *Limites* vol. 442, no. 90, AMRE; see also Nordman, "Buache de la Neuville," 105–06.

⁷⁴ Jacques Godechot, *La Grande Nation: L'Expansion révolutionnaire de la France dans le monde de 1789 à 1799*, 2 vols. (Paris, 1956), 1: 65–76.

⁷⁵ See T. C. W. Blanning, *The Origins of the French Revolutionary Wars* (London and New York, 1986), esp. chap. 2, for a useful discussion of the War of the First Coalition as a continuation of Old Regime foreign policy.

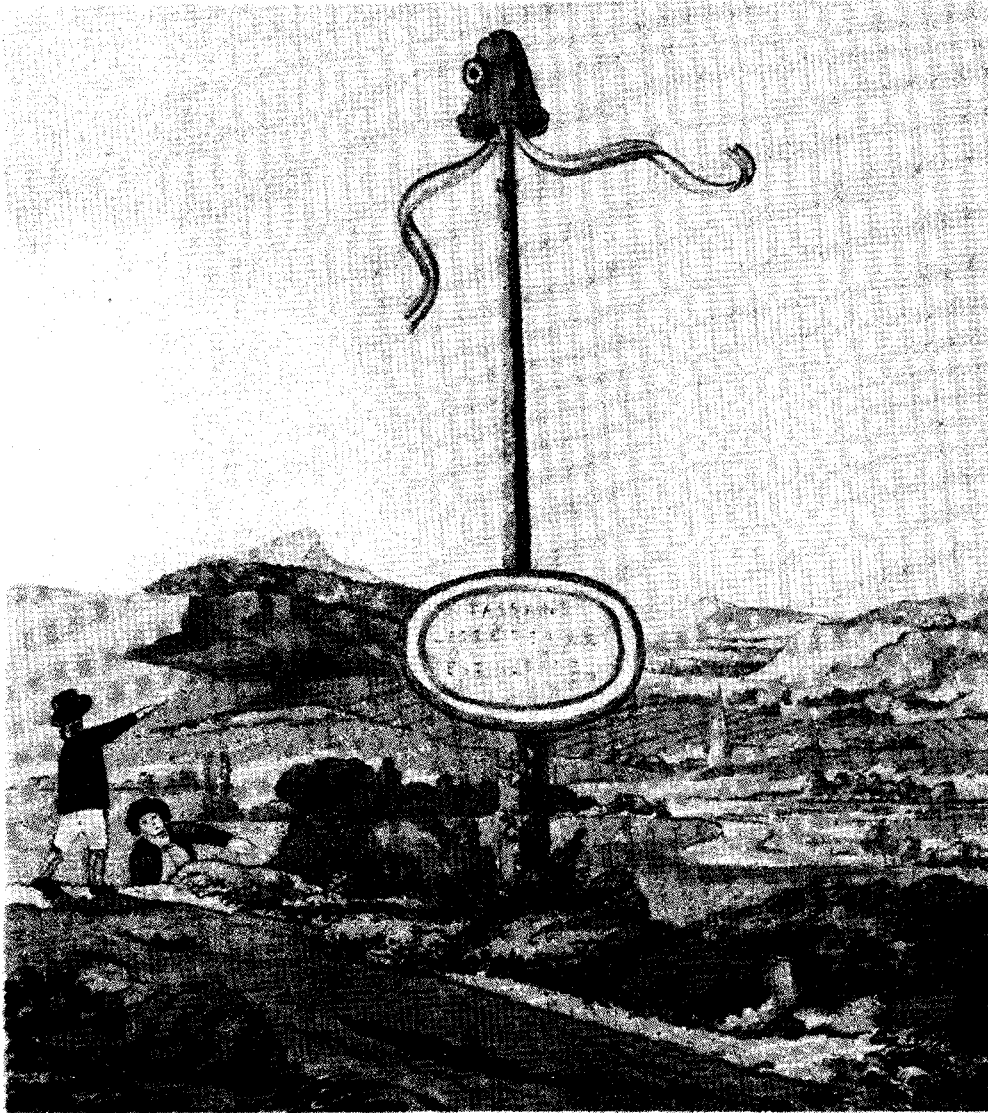


FIGURE 5: Johann Wolfgang Goethe, *the French boundary near the Moselle River*, watercolor (1790). The inscription on the liberty tree that serves as boundary marker reads, "To passers-by: this land is free." N. Hampson, *The First European Revolution* (New York, 1969), plate 70.

Valmy and Jemappes in September and November 1792, the revolutionary armies occupied Savoy, Nice, Belgium, and part of the left bank of the Rhine.

The National Convention was deeply divided on what to do with these conquered territories, some favoring annexation, others, like Camille Desmoulins, fearful "of looking like kings by chaining Savoy to the Republic."⁷⁶ In the winter of 1792–1793, deputies from the Girondin party, urging annexation, invoked France's

⁷⁶ Quoted in Godechot, *La Grande Nation*, 1: 77.

natural boundaries as one of several arguments justifying French annexation of the occupied territories. It is important to stress that the principal arguments for these annexations lay in the doctrine of popular sovereignty and a freely expressed desire to be united to France. The argument, put forth by the abbé Henri-Baptiste Grégoire, that “[c]easelessly the Alps have pushed [Savoy] into the French domain, and the order of nature would be contradicted if their governments were not the same,” was secondary to his major claim that the “reunion” originated as “the free and solemn expression of the near totality of the communes.”⁷⁷ Both Lazare Carnot, who urged the Convention to “reunite” Monaco, Schomberg, and other “neighboring communes,” and Danton, who argued in favor of annexing Belgium, used the trope of France’s natural and historical frontiers—but only after establishing the “free consent” of the countries in question.⁷⁸

Once again, the political claims to France’s Rhine frontier came from the Rhineland itself, where interest and principle neatly coincided: minority Patriot groups mobilized “public opinion” with the slogan of France’s natural limit, urging the annexation of their jurisdictions to France. The Prussian-born but Paris-based banker and revolutionary, Anacharsis Cloots, had published in 1785 the *Wishes of a Gallophile*, arguing that “the Rhine river is the natural boundary [*limite naturelle*] of the Gauls,” and, during 1792, he became an outspoken advocate of the program to annex. In November 1792, it was an idea whose time had come.⁷⁹ The Girondins needed a justification for annexation that was based neither on “conquest” (like the Old Regime monarchy) nor exclusively on expressions of popular support (unlikely during conditions of occupation). The partisans of the Rhine boundary used the image of a natural boundary to justify the bounded and limited quality—and defensive character—of French expansion under the Republic; they invoked the Rhine as France’s *limite*, not its *frontière naturelle*.

The idea of the French expansion to the Rhine, initially linked to the Girondins and their expansionist foreign policy, became part of a revolutionary platform during the course of the wars against the European coalitions, even when the Girondins failed to maintain power in the National Convention. Robespierre and the Montagnards opposed the use of annexations to achieve France’s natural boundaries, as well as the establishment of “sister republics” beyond them, but the idea of the “fatherland in danger” brought with it claims to the defensive “barrier of the Rhine.” The idea of the Rhine thus became central in two ways. Politically, it was the certificate of republican patriotism: to deny France’s claim to the Rhine was to be identified with the Old Regime monarchy and its frontiers.⁸⁰ More dramatically, the Rhine boundary, which had been invoked indiscriminately in revolutionary speeches as a boundary “marked by nature,” and as one of the “natural boundaries of ancient Gaul,” became the Rhine “barrier,” a symbol of

⁷⁷ *Le Moniteur universel*, 14, no. 333 (November 28, 1792): 585–88.

⁷⁸ Lazare Carnot, in *Le Moniteur universel*, 15, no. 48 (February 17, 1793); Danton, in *ibid.*, 15, no. 32 (February 1, 1793). All of the annexations were, in theory, to be ratified by popular vote. None of the decrees annexing Savoy (November 27, 1792), Nice (January 31, 1793), Monaco (February 15, 1793), or Belgium and nearly a hundred communities in the Rhineland (spring, 1793) mentioned the principle of France’s natural boundaries; on these, see T. C. W. Blanning, *The French Revolution in Germany: Occupation and Resistance in the Rhineland, 1792–1802* (Oxford, 1983), 68–69; the texts of most of these decrees may be found in G. F. de Märtens, *Recueil des principaux traités d’alliance, de paix, de trêve, de neutralité, de commerce, de limites, d’échange, etc., conclus par les puissances de l’Europe tant entre elles qu’avec les puissances et Etats dans d’autres parties du monde depuis 1761 jusqu’à présent*, 4 vols. (Göttingen, 1791–1801), 6: *passim*.

⁷⁹ Zeller, “La Monarchie d’Ancien Régime,” 329–31.

⁸⁰ Sorel, *L’Europe et la Révolution française*, 4: 174–86.

strategic defense against Prussian and Austrian aggression. Shorn of both historical and geographical determinations, natural boundaries in general and the Rhine in particular were valued for their perceived instrumental and functional role. That role became apparent during the summer of 1795, when a merchant from the Rhineland sponsored an essay competition on “whether it is in France’s interests to push back its boundaries to the Rhine.” Although an illustration gracing the published volume of affirmative responses invoked—in classical form—the descendants of the Gauls reclaiming their ancient limits, most of the essays made only brief allusions to Caesar and Gaul. Instead, the arguments for French annexation were founded principally on the strategic value of the Rhine for the security of the Republic (although several authors commented on the economic benefits of annexing the wealthy districts on the left bank, as well as the benefits of French citizenship that would accrue to the inhabitants themselves).⁸¹ (See Figure 6.) The perception of the Rhine as a natural barrier was the condition for the revolutionary government’s definition of its military and diplomatic goals.

The Directory made the program of the Rhine its own and worked consistently both in war and diplomatic negotiations to establish the Rhine as its boundary “marked by Nature.” Philippe-Antoine Merlin de Douai, the abbé Emmanuel-Joseph Sieyès, and Jean-François Reubell—the last of whom, native of Alsace, became in November 1795 one of the five members of the Directory—emerged as the spokesmen of the cause. Carnot, who had once supported the doctrine, led the opposition and advocated a set of “constitutional limits”—the territories between the boundaries of 1789 and the Meuse that could be formally annexed without revision of the constitution and in which plebiscites had expressed the will of the peoples to become part of France.⁸² But with Carnot’s exclusion from the Directory in March 1795, the partisans of France’s natural frontiers achieved a theoretical victory, however nominal in practice. The Basel Treaty of July 1795 with Spain named the “watershed of the Pyrenees Mountains” as dividing France and Spain, while the secret articles of the Basel Treaty with Prussia recognized the left bank of the Rhine as French territory. The definitive settlement of the Rhine question had to await the general peace with Austria. In 1797, the Peace of Campo Formio gave Austrian consent to French annexations, but it did not commit the Holy Roman Empire. Over the next year, a congress at Rastatt attempted to settle the compensations and indemnities of the left-bank princes, but it was unable to resolve the issues before the War of the Second Coalition had begun.⁸³ Thereafter, France’s Rhine frontier against the Batavian Republic was a boundary to be achieved, then—during the reign of Napoleon—surpassed as the limit of France.

THE FALL OF NAPOLEON and the Paris Treaty of May 1814 reduced France to its boundaries of 1792; following Napoleon’s “Hundred Days” and the defeat at Waterloo, France lost its post-1789 acquisitions (except Montbéliard, Mulhouse, the Comtat Venaissin, and Avignon). The Restoration Monarchy returned the left bank of the Rhine to the German Confederation in 1815, thus turning France’s

⁸¹ Georges-Guillaume Boehmer, ed., *La Rive gauche du Rhin: Limite de la République française, ou Recueil de plusieurs dissertations, jugées dignes des prix proposés par un négociant de la rive gauche du Rhin . . .* (Paris, an IV [1795–96]).

⁸² Albert Sorel, “Le Comité de Salut Public et la question de la rive gauche du Rhin en 1795,” *Revue historique*, 18 (1882): 318–19; Godechot, *Grande Nation*, 2: 89–92.

⁸³ Blanning, *French Revolution*, 79–80.



FIGURE 6: The descendants of the Gauls reclaiming their ancient limits. Georges-Guillaume Boehmer, *La Rive gauche du Rhin: Limites de la République française* (Paris, an IV[1795–96]).

claim to the Rhine into "a lost and infinitely desirable goal."⁸⁴ By 1833, even a royalist like Chateaubriand could idealize the old republican frontier, recalling how Gaul had once worn the "blue scarf of Germany," the Rhine.

Napoleon and the Republic before him had realized the dream of several of our kings and especially Louis XIV: as long as we have not occupied our natural frontiers [*frontières naturelles*], there will be war in Europe because the interest in her conservation pushes France to seize the limits necessary to her national independence. Here we have planted the trophies to reclaim at the right time and place.⁸⁵

This notion of France's destiny reconciled opposing political regimes, a fact emerging most clearly among the historians and publicists of the July Monarchy, who formulated and disseminated the "doctrine." The idea of natural frontiers offered a common ground to partisans of the revolution (who claimed the Rhine boundary of 1795) and those of the Restoration (who identified themselves with the monarchy's limits of 1789). It was Augustin Thierry, writing in the 1830s, who seems to have offered the first systematic version of the concept:

One can say that even when drunk with military success, and despite the crises of ambition which peoples as well as individuals suffer, the nation firmly and consistently wanted only the maintenance of our natural boundaries [*limites naturelles*]. Whatever our fortune, good or bad, the idea of taking them back was never lost: it is profoundly national and profoundly historical.

Thierry wrote of an unchanging aspiration from the "living fountain of Gaul, independent or Roman" through Louis XIV and the revolution, which was then "unfortunately" if temporarily surpassed.⁸⁶

Zeller clearly documented that, by the middle decades of the nineteenth century, historians of all political persuasions had adopted the doctrine as a given. The idea entered into the consciousness of a wider reading and schooled public in the most popular histories and textbooks of mid-century—among them Henri Martin's *History of France*, which Zeller called "the historical Bible of the middle classes." And Zeller's explanation seems, this time, exactly right. The profound continuity of French national history offered by the idea of natural frontiers provided an expanding reading public with "a lesson of sacred union," a symbol of national unity.⁸⁷ It was this public opinion that made it possible for the government of Adolph Thiers in 1840, thwarted in its imperialist adventure in the Middle East, to put pressure on another front closer to home. Thiers and the left-wing press both sponsored a propaganda effort, which included a pamphlet by historian Edgar Quinet, to reclaim the Rhine frontier of France.⁸⁸

At the same time, Zeller did not consider the extent to which the symbolic value of natural frontiers was lessened as the ideas of nationality and self-determination, introduced by the revolution, were elaborated in nineteenth-century historical writing. Natural boundaries were not considered a meaningful framework of national identity unless they could be linked to a voluntary identification with the French nation. Fustel de Coulanges, in his oft-quoted response to the German historian Theodor Mommsen

⁸⁴ Zeller, "La Monarchie d'Ancien Régime," 333.

⁸⁵ Quoted in Nordman, "Des limites d'Etat," 52.

⁸⁶ Quoted in Zeller, "Histoire d'une idée fausse," 118–19; on Thierry and contemporary historical writing, see J. Walch, *Les Maîtres de l'histoire, 1815–1850: Augustin Thierry, Mignet, Guizot, Thiers, Michelet, Edgar Quinet* (Geneva-Paris, 1986).

⁸⁷ Zeller, "Histoire d'une idée fausse," 120, 124–27.

⁸⁸ H. A. C. Collingham, *The July Monarchy: A Political History of France, 1830–1848* (London, 1988), 231–32.

in 1870, echoed Jules Michelet in claiming that "what marks a nation is neither race nor language. Men feel it in their hearts that they are a single people when they have in common thoughts and interests, affections, remembrances, and aspirations."⁸⁹ The developing image of national identity in nineteenth-century historical writing, as within French political culture more generally, thus retained the complexity and tension of the simultaneous claims of natural boundaries and those of nationality. It was, at least in part, a tension between the demands of a state, seeking its strategic and military goals, and those of a nation, defined by its affective bonds, past history, and voluntary self-determination.

Natural frontiers continued to be identified as the goal of military strategy, and *mémoires* written by officers and military engineers throughout the nineteenth century continued to identify the interests of French foreign policy with natural frontiers, regularly invoking the more expansionist and zonal sense of the phrase. "The natural frontier [*frontière naturelle*] is the true military frontier," wrote General Pelet about the "frontier of Italy or of the Alps" in 1819.⁹⁰ Under Napoleon III, the "politics of nationality" and the concern with natural frontiers intersected in unexpected ways within French foreign policy in the Alps and along the Rhine. The idea of natural frontiers provided the strategic arguments presented to the minister of foreign affairs and adopted as policy by Napoleon III for France's acquisition of Savoy. Savoy had been lost in 1815 and reincorporated into France in the treaty of March 24, 1860, between Napoleon III and the Piedmontese prime minister Count Camillo Cavour. As Paul Guichonnet has shown, Napoleon III himself never undertook the annexation based on a politics of nationality, but he did argue for the military advantages of France's natural frontier along the Alps: "In the presence of this transformation in Northern Italy which gives to a powerful state all the passages of the Alps, it is my duty, for the security of our frontiers, to demand the French watershed." Military strategy—and the revival of the older, seventeenth-century image of natural frontiers—determined the arguments for the acquisition of Savoy; the Romantic invocation of national self-determination served as "an ideological cover [and] a sentimental justification."⁹¹ As for Napoleon's German policy, the notion that France pursued its "traditional" expansion to the Rhine during the German crisis of 1865–1866 has recently been shown inadequate, since Napoleon was less concerned with France's acquisitions of territories on the left bank of the Rhine itself than with a desire to see Germany reorganized in a way that would suit French diplomatic concerns with a balance of power. If anything, Napoleon pursued the politics of nationality "in promoting the cause of German nationalism as represented by Prussia, in the best tradition of the *Idées Napoléoniennes*."⁹²

After the loss of Alsace in 1871, and increasingly after its recovery in 1918,

⁸⁹ Quoted in Nordman, "Des limites d'Etat," 56. The French model of nationality in the nineteenth century stood opposed to the German model, which from the sixteenth-century cosmographer Sebastian Münster to Johann Gottlieb Fichte's *Addresses to the German Nation* in 1807 stressed ethnocultural identity, and especially the German language, as the foundation of national character. Throughout the nineteenth century in Germany, the attempt to trace German linguistic frontiers explicitly made the Rhine into a "German stream," a vision realized after 1871; see Norman J. G. Pounds, "France and 'les limites naturelles,' from the Seventeenth to the Twentieth Centuries," *Annals of the American Association of Geographers*, 44 (1954): 57–58.

⁹⁰ "Rapport sur la frontière des Alpes," MR 1207, no. 1, MG AAT.

⁹¹ Paul Guichonnet, "Théorie des frontières naturelles et principes des nationalités dans l'annexion de la Savoie à la France, (1858–1860)," *Revue des travaux de l'Académie des sciences morales et politiques*, 113 (1960): 23; quote on 21.

⁹² For a reconsideration of the "traditional" view, exemplified by Hermann Oncken, *Napoleon III and the Rhine*, trans. Edwin H. Zeydel (New York, 1928), see E. Ann Pottinger, *Napoleon III and the German Crisis, 1865–1866* (Cambridge, Mass., 1966), quotation on 209.

natural frontiers—by then understood exclusively as France's claim to the left bank of the Rhine—gained new meaning within a coherent nationalist discourse.⁹³ The nationalism evident in the continued emphasis on France's *frontières naturelles* was grounded in the synthesis of Third Republic politicians and historians—like Sorel and Mathiez—who reconciled the political antithesis of monarchy and republic, of *ancien régime* and revolution. But the nationalization of natural frontiers had a particular significance in the 1920s and 1930s, as the ideas were increasingly implicated in the growing militarization of the Rhineland. To Zeller, the journalistic, scholarly, and textbook invocations of natural frontiers were part of a French “ideology” invented to match the German “ideology” that the Holy Roman Empire was a direct prolongation of the Roman empire. He saw this “conflict of ideologies, not interests” at the heart of French-German antagonism. That opposition had “no profound rationale, only an accidental character: if there is a lesson to learn from the long history of their relations,” Zeller concluded in his 1932 study, “it is in the first place this one.”⁹⁴

Less than a decade later, Zeller himself experienced the all-too-real consequences of the twentieth-century “doctrine.” The Maginot Line, that “impregnable” system of connected, concrete bunkers along France's northeastern boundary, owed much to Vauban and the seventeenth-century idea of an enclosed and defensible space, itself framed by an idealized notion of France's natural frontiers.⁹⁵ But, in the summer of 1940, the belief in a limited and defensible territory proved as illusory as the medieval notion of the “four rivers,” and Zeller—injured in 1916 and unable to serve at the front—left Strasbourg with the “exiled” French university to return to his hometown of Clermont-Ferrand. There he continued to lecture until censored in 1943 by the Vichy government for subversive remarks about Joan of Arc. He returned to Strasbourg in 1945 and the next year was called to Paris, where he finished his career at the Collège de France, writing extensively on French foreign policy but never explicitly returning to the project of demystifying the “false idea” of France's natural frontiers.

THE IDEA OF NATURAL FRONTIERS was a powerful, recurrent image in the shifting repertoire of French political culture. The meaning of natural frontiers—defined geographically or historically, as a bellicose *frontière* or a restrictive *limite*, as a general description or a specific political claim—changed dramatically, as did the different political functions of the idea during three centuries of state building in France.

In the seventeenth century, the idea of natural frontiers acquired a historical cast, as it helped shape a concept of a unified state; although neither Richelieu nor Louis XIV intended literally to restore France to Gaul's natural frontiers, the idea nonetheless served their policies and occasionally framed the military strategies of the emerging territorial state. In the eighteenth century, the image of natural boundaries lost its bellicose and historical resonance as the idea became part of an enlightened program of political reform, itself brought on by the international

⁹³ Jean-Marie Mayeur, “Une Mémoire-frontière: L'Alsace,” in Nora, ed., *Les Lieux de mémoire*, *La Nation*, 2: 63–95.

⁹⁴ Zeller, *La France et l'Allemagne*, 202–07. On the University of Strasbourg during the 1930s, see John E. Craig, *Scholarship and Nation Building: The Universities of Strasbourg and Alsation Society, 1870–1939* (Chicago, 1984); and Livet, “L'Institut et la chaire,” 208–09.

⁹⁵ On the Maginot Line, see Pierre Rocol, *Deux Mille Ans de fortification française*, 2 vols. (Limoges and Paris, 1973), 1: 32–43.

weakness of France. The French Revolution preserved the Enlightenment interpretation while politicizing natural frontiers and created—in the context of France's expansionist Rhineland policy—a political program and an ideological doctrine. Finally, in the nineteenth century, the idea was reinterpreted as a symbol of national unity reconciling the principles of monarchy and republic, and was widely diffused among a growing middle-class reading public, especially as it regained a militaristic connotation between the two world wars.

The image of France's natural frontiers survived Gaston Zeller's announcement of its death during the 1930s and World War II, although its power has lessened. Invocations of the "doctrine" may still be found today, but textbooks, dictionaries, and popular histories of France have shifted away from descriptions of France's natural frontiers and, in their place, offer more neutral accounts of France's hexagonal shape. The idea of France as a hexagon first appeared in French geography texts of the 1850s, during debates over imperial expansion at the expense of Piedmont; but, while history and geography textbooks of the Third Republic increasingly referred to France's hexagonal shape, it was only in the 1950s that the hexagon came into its own. Though a relative latecomer to the repertoire of symbols of national identity, the hexagon came to occupy a central place in the visions of French unity, suggesting the qualities of harmony, balance, stability, and permanence.⁹⁶ The relative neutrality of the hexagon offered a double compromise. On the one hand, the hexagon represented a conventional and a natural unity, an identity founded at once on culture and on nature. On the other hand, it balanced the claims of a national community to self-determination with those of a state seeking defensive strategies. Given its potential for multiple interpretations, it is not surprising that the hexagon became a disputed symbol within the political culture of postwar France.⁹⁷

Among historians of Old Regime and revolutionary France, Zeller's devastating critique of the "doctrine" of natural frontiers has become a commonplace—a received and unquestioned idea—while historians of the nineteenth and twentieth centuries have yet to rediscover its role in the symbolic construction of France. Zeller was too quick to dismiss as mere ideology the variety of meanings of natural frontiers in the history of French expansion and the construction of national identity. Beyond a simple opposition of interest and ideology, the category of natural frontiers can be situated within the shifting cultural idioms of French state building. Beyond France, within German and European political cultures, and in the colonial and postcolonial worlds, the reconsideration of natural frontiers should yield new ways of talking about the construction and deconstruction of national states.

⁹⁶ For divergent interpretations on the appearance and diffusion of the hexagon, see Eugen Weber, "L'Hexagone," in Nora, ed., *Les Lieux de mémoire, La Nation*, 2: 97–116; and Nathaniel B. Smith, "The Idea of the French Hexagon," *French Historical Studies*, 6 (1979): 139–55.

⁹⁷ During France's "civilizing mission" in Algeria and Indochina during the late 1950s and early 1960s, French colonials bestowed the epithet "hexagonals" (*hexagonaux*) on those of "metropolitan France," and in the late 1960s, the right-wing Gaullist party took over the image of the hexagon in their electoral campaigns, while the left distanced itself from a symbol of nationalism and French hegemony. See Smith, "Idea of the French Hexagon," 150–51; and Stanley Hoffman, et al., *In Search of France* (Cambridge, Mass., 1963), 227, 346, 356.

Review Article
On the Latent Illiberalism of the French Revolution

ISSER WOLOCH

A Critical Dictionary of the French Revolution, François Furet and Mona Ozouf, eds., Arthur Goldhammer, trans. (Cambridge, Mass.: Belknap Press, Harvard University Press, 1989), 1,063 pp.

SOON AFTER the appearance of *A Critical Dictionary of the French Revolution*, Conor Cruise O'Brien—a tough-minded critic and ideological *franc-tireur*—published a glowing review under the title “The Decline and Fall of the French Revolution.” The headline for his article on the cover of the *New York Review of Books* was even more pointed: “Goodbye, French Revolution!”¹ The title may not have been O'Brien's conceit, and the tag line certainly was not, but both conformed closely enough to the tenor of the article and, in certain ways, to the thrust of the book itself. The sentiment is meant in at least two ways: that the legacy of the French Revolution does not and should not intrude any longer on contemporary political consciousness in France or, presumably, anywhere else. And that a proper understanding of the French Revolution ought to result in its demotion, perhaps even banishment, from the inspirational pantheon of the liberal-democratic tradition. Insofar as the *Critical Dictionary* is probably the weightiest and most commanding product of the recent bicentennial, this view might well come to represent the net historical verdict emerging from that notable event. Indeed, the novel arguments and insights in this volume cannot but modify certain common attitudes and assumptions about the French Revolution. It would be a pity, however, if it swept the field clean.

First, a word about the bicentennial context of this work. While the French government progressively scaled down its public commemoration of the bicentennial—having started with plans for a universal exposition and ended instead with the thematically vacuous *défilé* on July 14—the scholarly side of the observance flourished under the leadership of Michel Vovelle. For the academic branch of the French Bicentennial Commission, it was an article of faith to encourage as much provincial and international scholarly activity as possible. Six entire issues of its journal-sized publication did nothing but list the hundreds of colloquia and symposia being planned across France and around the world.² Such overkill had its ludicrous side, as French revolutionary scholars briefly became tireless jet-setters.

¹ François Furet and Mona Ozouf, eds., *A Critical Dictionary of the French Revolution*, Arthur Goldhammer, trans. (Cambridge, Mass., 1989), 1064, xxii. The French edition was published by Flammarion in late 1988. O'Brien's review appeared in *The New York Review of Books*, February 15, 1990.

² 1789–1989: *Bicentenaire de La Révolution française* (6 nos.: 1983–89).

Some of these conferences, however, proved to be exceptionally rich, as the appearance of their published proceedings is beginning to demonstrate. All this activity culminated at the World Congress on the Bicentennial, held for seven days on the eve of July 14 in Paris, whose theme was the "Image of the French Revolution"—its reverberations around the world and across two centuries, as well as in its own time and place. Scholars from about fifty countries presented over three hundred papers in six parallel working sections, and during the last two days a group of able *rapporteurs* reflected on the import of these papers. Naturally, the significance of the topics and the quality of the papers varied immensely, some being incontestably trivial or derivative. Nor were the marathon sessions of presentation particularly inspiring. But participants still shared the sense of a historic occasion and grand scholarly undertaking. The four volumes of proceedings offer a massive harvest of food for thought on what the French Revolution may have meant to different kinds of people in its own day and after.³

Hundreds of symposia and thousands of reports on monographic research do not automatically add up to progress. But the compelling thematic choices in some of these conferences and the range of sound, and occasionally innovative, scholarship in their papers challenge specialists in the field to incorporate the results of their colleagues' labors.⁴ Paradoxically, the volumes of synthesis occasioned by the bicentennial—whatever their merits or shortcomings—are in that sense pre-bicentennial works that have not profited from the effusion of scholarship that culminated in 1988–1989.⁵ The same reservation applies to the *Critical Dictionary*, the veritable thunderbolt of bicentennial publications.

IF THE MEEK ARE TO INHERIT THE EARTH, François Furet will be left without patrimony. His entire career as a historian of the French Revolution has been marked by remarkable boldness and a genius for altering the terms of discussion in the field. Along with George Taylor's seminal *AHR* essay attacking the linkage between a capitalist bourgeoisie and the French Revolution, Furet's explosive 1971 article, "The Revolutionary Catechism," launched the revisionist enterprise on its triumphal march. Combining uninhibited polemic with probing speculation, Furet denounced the stultifying impact of the prevailing celebratory, Marxist-Jacobin historiography and insisted that reinterpretation was even more important than the multiplication of further research. *Interpreting the French Revolution*, his collection of four essays, including the "catechism" article and essays revalorizing Alexis de Tocqueville and Augustin Cochin as thinkers who pointed the way, rounded out

³ *L'Image de la Révolution française*, 3 vols. (Oxford and Paris, 1989), with a fourth volume of abstracts and the comments of *rapporteurs* (1990). In a perplexing turn of events, not a single French contributor to the *Critical Dictionary* chose to participate in the World Congress.

⁴ The published proceedings of conferences with which I am familiar include F. Lebrun and R. Dupuy, eds., *Les Résistances à la Révolution* (Paris, 1987); *La Révolution française et l'ordre juridique privé: Actes du Colloque d'Orléans*, 2 vols. (Paris, 1988); *Pratiques religieuses, mentalités et spiritualités dans l'Europe révolutionnaire (1770–1820): Actes du Colloque de Chantilly* (Paris, 1988); *Les Pratiques politiques en province à l'époque de la Révolution française: Actes du Colloque de Montpellier* (Montpellier, 1988); *La Révolution française et le Monde rural: Actes du Colloque de la Sorbonne* (Paris, 1989); M. Vovelle, ed., *Paris et la Révolution* (Paris, 1989); *Les Femmes et la Révolution française: Actes du Colloque de Toulouse* (Toulouse, 1990); A. Forrest and P. Jones, eds., *The French Revolution: Town, Country, and Region* (forthcoming).

⁵ For a review of several works that are in this sense "pre-bicentennial," see Jack Censer, "Commencing the Third Century of Debate," *AHR*, 94 (December 1989): 1309–25.

Phase One of his project.⁶ Phase Two was a kind of preemptive coup on the bicentennial. Passing from criticism and speculative interpretation to concrete historical analysis, Furet—in collaboration with Keith Baker and Colin Lucas—organized long in advance of the bicentennial a sequence of conferences on the “The French Revolution and the Creation of Modern Political Culture.” The substantial papers for these carefully planned symposia (at Chicago in 1986, Oxford in 1987, and Paris in 1988) circulated in advance and were published quickly on the heels of each conference. Thus, by 1989, the two volumes already in print gave a distinctive prominence to the political and ideological topics that these conferences had placed in the spotlight.⁷ On a parallel track came the *Critical Dictionary*.

At first glance merely one of several large-scale reference works produced by an array of publishers for the bicentennial, the *Critical Dictionary* actually stands alone as a carefully meditated *machine de guerre*. Its two nearest rivals are far more modest in their ambitions, formats, and thrusts. Representing the so-called traditional camp is the lavish *Dictionnaire historique de la Révolution française* (1989). Begun under the aegis of Albert Soboul, it was carried out after his death by collaborators based primarily in the Institut de l'Histoire de la Révolution Française at the Sorbonne. It is a conventional, concise, and wide-ranging reference encyclopedia. Its biographical articles, for example, cover the Mirabeaus and Dantons as well as lesser-known figures such as the Jacobin editor Vatar or the back-bench *conventionnel* Bouquier. Here one can look up most of the revolution's pivotal events and institutions, as well as conceptual subjects such as Unity/Indivisibility or Direct Democracy. Occasionally, it offers a state-of-the-question review, as in articles on the Chouan insurgency by Roger Dupuy and on the Vendée by Claude Petitfrère, the leading French experts on those subjects. Packed full of information in readily accessible form, it is an extremely useful tool for which all students and scholars should be grateful.

L'Etat de la France pendant la Révolution (1789–1799), edited by Michel Vovelle (1988), is more innovative conceptually, with articles grouped under such intriguing headings as: “Contextes,” “Les Temps de la Vie,” “Eux et Nous,” “La France des Régions,” and “Acquis et Débats.” The range of subjects is sprawling and imaginative, “an invitation to revisit the France of 1789–1799 from the perspective of a trip across multiple aspects of the life of the people—of *all the people*, rich or poor, rural or urban, revolutionary or oppositional.”⁸ With the sense that the revolution is anything but *un objet froid*, the contributors are most interested in the human environments of the period: its material civilization, its new patterns of sociability, and its imagery. Although the volume suggests the richness and continuing vitality of this field, its potential impact is limited by the cramped nature of its typical two or three-page articles.

A *Critical Dictionary of the French Revolution* is a different matter entirely. Its

⁶ François Furet, *Penser la Révolution française* (Paris, 1978), Elborg Forster, trans. as *Interpreting the French Revolution* (Cambridge, 1981). George V. Taylor, “Noncapitalist Wealth and the Origins of the French Revolution,” *AHR*, 72 (April 1967): 469–96. For a synthesis reflecting this phase of the revisionist project, see William Doyle, *Origins of the French Revolution* (Oxford, 1980).

⁷ Keith Baker, ed., *The Political Culture of the Old Regime* (Oxford, 1987); and Colin Lucas, ed., *The Political Culture of the French Revolution* (Oxford, 1988). The first of these volumes is analyzed in two review articles: Sarah Maza, “Politics, Culture, and the Origins of the French Revolution,” *Journal of Modern History*, 61 (1989): 704–23; and Jack Censer, “The Coming of a New Interpretation of the French Revolution,” *Journal of Social History*, 21 (1987): 295–309.

⁸ Michel Vovelle, ed., *L'Etat de la France pendant la Révolution (1789–1799)* (Paris, 1988), 3–4.

ambitions are much larger, its focus much sharper, and its likely impact much greater. (That an English translation was under way well before it was even published in France is simply one mark of those ambitions.) In a long preface, the two editors and principal contributors, François Furet and Mona Ozouf, explain that

We have produced a "dictionary" in the sense made familiar by the Enlightenment. Its principle: a set of key words, which suggest not only the state of current scholarship but even more a shift in the nature of the questions posed. Its objective: to recover both the strangeness and the disruptive force of the founding event of modern French history. Its unity: the stress placed on the political event and its creative capacity.⁹

This is not entirely candid. The unity—which is indeed striking—comes as well from the recruitment of a like-minded team of historians and political theorists drawn primarily from the Ecole des Hautes Etudes en Sciences Sociales and its satellite, the Institut Raymond Aron. Like Denis Diderot's *Encyclopédie*, it is the product of an intellectual kinship group or coterie, and it resembles that work as well in being anything but a mere reference book or inventory of received information. Using the same techniques of cross-reference and reiteration adopted by Diderot, its intention is to change the current way of thinking. For this, the editors created a powerful and appealing format: a freewheeling essay form allowing full-bodied displays of originality and erudition, flavored every now and then by veiled polemical argument. What a luxury to assess the Montagnards or Girondins in fifteen long pages of text; to range over Robespierre's career without being bound by any requirements of chronology or coverage; to ponder the idea of equality or of sovereignty by virtually creating one's own taxonomy and sense of the subject! The ninety-nine essays are grouped into five sections: Events, Actors (Individuals and Groups), Institutions and Creations, Ideas, Historians and Commentators, which, of course, overlap. If political ideas are at the volume's core, "the ideas at issue here did not arise out of purely theoretical debate. They were put into practice and embodied in institutions. They were disseminated through a pedagogy. They were modified by events. They became issues in political combat."¹⁰

The preface to the *Critical Dictionary* attacks the bloc model of the revolution, with its demand for "unconditional piety and compulsory devotion": "Even in its classical sense, the French Revolution exhibits no unity as regards its heroes or leaders, its actors' justifications, or its political forms . . . and the reader will learn how much the diversity of the event exceeds the unity of the concept." But the editors also reject the proposition "that the events of 1789 and 1793 were radically heterogeneous . . . The entire *Critical Dictionary* is a protest against this surgical distinction. One of its contributions, perhaps, is to have restored the year 1789 to a central position . . . anticipating all that was to come, from the radical escalation of the Jacobin period to the sober retreat of the Thermidorian era."¹¹ The volume promises to the reader and delivers an intellectual adventure that, if my experience is typical, will stimulate, enlighten, and exasperate in about equal measure.

"HISTORIOGRAPHY OCCUPIES A FUNDAMENTAL PLACE in this book—it can be found in nearly all the articles," explains the preface. Although it comes fifth and last, the

⁹ Furet and Ozouf, *Critical Dictionary*, xiv.

¹⁰ Furet and Ozouf, *Critical Dictionary*, xv.

¹¹ Furet and Ozouf, *Critical Dictionary*, xviii–xviii.

section on historians and commentators is logically the very source of the volume's critical stance. To be sure, the outlines of this classic historiography are familiar fare in the classroom—the liberal post-mortems of the Restoration; the patriotism and passion of Jules Michelet and the generation of 1848; and the politics of Third Republic historiography. The subject has been frequently mapped as a kind of ritualistic prologue or predigested curriculum, as in John McManners's beautifully turned article in the *New Cambridge Modern History*, whose epigrammatic insights seem inexhaustible.¹² Such surveys, however, do not usually suggest that this galaxy of classics offers any relevant inspiration to scholars today. The moralizing and Olympian abstraction of the nineteenth-century historians, not to mention their adherence to a Paris-centered narrative tradition, seemed to place them in another universe.

For the editors of the *Critical Dictionary*, however, nothing could be further from the truth: "The development of revolutionary historiography contradicts the conventional wisdom that the longer ago an event took place, the 'truer' our knowledge of it is. The most profound questions about the Revolution were posed very early."¹³ Commentators such as Edmund Burke and Benjamin Constant, and nineteenth-century historians from François Guizot and Louis Blanc to Hippolyte Taine and Jean Jaurès, regardless of their consuming passions for or against the revolution, are valued by the editors for their philosophical or critical approach to the subject. At its best, the section on historians is reminiscent of Pieter Geyl's study of Napoleonic historiography, where the works of nineteenth-century historians, for all their extreme subjectivism, are made to speak meaningfully to contemporary readers.¹⁴ In contrast, the editors believe that an uncritical adulation of the revolution cloaked in the garb of positivism and scientific method generally weakens the value of academic historiography in the twentieth century.

The editors' approach is most clearly visible in the article on Edgar Quinet. This may indeed be an instance in which Furet has effectively rediscovered a figure ordinarily treated in the most cursory fashion. It is no mere whim that leads to Quinet's elevation, for Furet finds in him the consummate philosophical historian, a writer preoccupied with the question of why the reality of the revolution deviated so drastically from its professed ideals. Quinet, despite his republican sympathies, condemned 1793 uncompromisingly, seeing it as the complete negation of 1789. Quinet, in Furet's words, "criticized the Terror not so much for its violence as for its meaninglessness. It was an extermination process that operated in a vacuum and had no purpose other than to sacrifice individuals to the state. To that extent it revived absolutist tradition in a revolutionary mode. Not only did the guillotine fail to point the way to the future; it brought back the past."¹⁵ Quinet's explanation was highly idiosyncratic, having to do with the failure of the revolution to create a value system that would at once absorb and displace Christianity. For Furet, however, Quinet shows the way, first by his total rejection of the Terror, and second by depicting it as a resuscitation of absolutism. Without sharing Quinet's belief in the radical contrast between 1789 and 1793, Furet builds on Quinet's critique to create a new sense not only of 1793 but of 1789 itself.

¹² John McManners, "The Historiography of the French Revolution," *New Cambridge Modern History*, Vol. 8 (1965). See also Alice Gérard, *La Révolution française: Mythes et interprétations* (Paris, 1970).

¹³ Furet and Ozouf, *Critical Dictionary*, xvi.

¹⁴ Pieter Geyl, *For and Against* (New Haven, Conn., 1949).

¹⁵ Furet and Ozouf, *Critical Dictionary*, 998–99. Furet has written about Quinet at greater length in *La Gauche et la Révolution française au milieu du XIX^e siècle: Edgar Quinet et la question du Jacobinisme* (Paris, 1986).

Although it comes first alphabetically, the article titled "Academic History of the Revolution" logically concludes the reconsideration of nineteenth-century historiography. Here the tone shifts perceptibly, and the treatment becomes more impatient. Notwithstanding the occasional word of grudging praise, Furet's discussion of Alphonse Aulard, Albert Mathiez, and Georges Lefebvre is decidedly sour compared to the customary evocations of this venerable tradition. The introductory section of the essay is unexceptionable, as it notes the belated but definitive triumph of the Republic in the era of Jules Ferry and Léon Gambetta—a tamed, more-or-less consensual Republic whose revolutionary ancestry could now be celebrated without inhibition. Under these auspices, the revolution came to the Sorbonne, as "The Republic needed a historian who would rewrite the Revolution's annals for the benefit of all the children of France."¹⁶ This happened to coincide with the triumph of positivism in historical studies, the paradoxical ambition to make history into a science of the particular.

In the kind of orthodoxy that it spawned, Furet contends, such "scholarship was supposed to extend the range of knowledge and limit that of interpretation. It claimed the certainty of science and narrowed the scope of debate." Up to a point, this is a valid argument. It is certainly true that the scope of debate narrowed perceptibly in the era of Mathiez and Lefebvre and that, on the broadest level, interpretation tended to be formulaic and celebratory; carried to excess, the amalgam of adulation and positivism could indeed be a blight. But interpretation or understanding advances not simply by critically dissecting a familiar body of experience but by enlarging the landscape and by viewing it from new perspectives. Furet acknowledges that, in its ambition to extend our knowledge of the facts, the new academic historiography was successful. That is faint praise, however, and does not really address the creation of new perspectives (agrarian, social, economic, political) that developed in those decades of seemingly complacent orthodoxy. One might certainly take exception to Furet's respectful but less-than-enthusiastic discussion of Lefebvre, whose talent for recovering new levels of experience in the French Revolution was unsurpassed. Furet's real argument, in any case, is with the "thesis of circumstances," which received its most unqualified formulation by Aulard. Though acutely skeptical about Robespierre, Aulard was an ardent apologist for the revolutionary government of the Year II as an improvised response to dire circumstances, which "formed itself empirically from day to day, out of the elements imposed on it by the successive necessities of the national defense."¹⁷ Furet and Ozouf categorically refuse any commerce with that view. Circumstances and "revolutionary necessity" are for them utterly deficient as explanations, let alone justifications for what occurred in 1793–1794. One antidote for that view, they believe, is to look more closely at what was happening in 1789.

A PROVOCATIVE ARGUMENT about the radicalism of 1789 and its latent illiberalism stands at the heart of the *Critical Dictionary's* interpretive framework. According to Furet, despite liberal rhetoric and convictions, the men of 1789 reestablished a kind of absolute authority empowering the state at the expense of the individual citizen. This is to give a new inflection to the familiar argument of Tocqueville, who had allowed for a brief, if stillborn, liberal interlude in 1789–1791 before the author-

¹⁶ Furet and Ozouf, *Critical Dictionary*, 882.

¹⁷ Alphonse Aulard, *A Political History of the French Revolution*, 4 vols. (New York, 1901), vol. 3.

itarian, centralizing habit reasserted itself. The fateful choice in 1789 was the National Assembly's appropriation of complete sovereignty in the name of the people, compounded by the particular doctrine of representation that the Constituent Assembly embraced in the summer of 1789. The intoxicating dream of a unitary national will, present from the beginning, further diluted the sway of liberal values. For Furet, "the radical course of the Revolution after 1789 is less difficult to explain than the radicality of 1789 itself."¹⁸

The implications of this view are developed in the article on the Rights of Man—which would seem to be the terrain most hospitable to the liberal claims of the French Revolution. Unfortunately, Marcel Gauchet believes, at the very moment that the Assembly celebrated individual rights in August 1789, it was constrained by the need to establish the revolutionary state's legitimacy and authority and to dismantle the society of orders. The members of the Assembly therefore came "to invest Rousseauist categories with a central resolving role," weighted against the individual. Although he does not put it this way, Gauchet has noticed that, despite the Assembly's explicit rejection of the proposal to promulgate an accompanying Declaration of Duties, it effectively internalized one in the text of the Declaration of Rights. The Declaration synthesized the rights of man and the duties of citizens around the notion of positive law, which became a leitmotif in the final text. Specific liberties and rights (religion, thought, due process, fair trial) were always qualified: "the liberty that had just been proclaimed [was placed] under the jurisdiction of a law capable of broadening or narrowing its limits."¹⁹ Echoing a similar and more complex presentation in Ozouf's article on the idea of liberty, Gauchet explains that, for the Assembly, "the expression of liberty was inseparable from the affirmation of authority that extended it and served as its instrument. Hence there was no reason for them to attack the principle of oversight"—which, all things considered, seems a sensible approach by an Assembly that could be forgiven a measure of optimism at that juncture. But, for Gauchet, echoing Furet, "later radicalization did not innovate as much as it exploited possibilities present from the beginning."²⁰

The *Critical Dictionary*, however, is rarely content to leave well enough alone. Its besetting sin is the authors' tendency to carry insights and suggestive arguments to an extreme. The reader may judge by considering some of the concluding sentences in Gauchet's essay on the Rights of Man: "The need to translate individual autonomy into social power worked against preservation of individual rights through limitation of power. It was but a short step from the liberal inspiration to the authoritarian temptation. Thus we see why a revolution based on the theory of the rights of man failed to conceive or establish a regime to guarantee them . . . Thus the founding text . . . embodied in condensed form a concept of liberty that prevented its realization."²¹ "Realization," let alone "guarantees," would indeed be a tall order. Thinking like a political philosopher rather than a historian, perhaps, Gauchet is forgetting that such things are of necessity a process and a struggle, not a settled, irreversible outcome. And to the contention that "it was but a short step from the liberal inspiration to the authoritarian temptation," I would

¹⁸ Furet and Ran Halévi, "L'Année 1789," *Annales: E.S.C.* (January 1989): 21.

¹⁹ Furet and Ozouf, *Critical Dictionary*, 826.

²⁰ Furet and Ozouf, *Critical Dictionary*, 819. Compare Gauchet's book-length treatment: *La Révolution des Droits de l'homme* (Paris, 1988); A. DeBaecque, et al., *L'An I des Droits de l'homme* (Paris, 1988); and Philippe Raynaud, "La Déclaration des droits de l'homme," in Lucas, ed., *Political Culture of the Revolution*.

²¹ Furet and Ozouf, *Critical Dictionary*, 827–28.

make the obvious response that, on the contrary, it was a very large step or, better yet, a series of unanticipated steps in which circumstances (the veritable *bête noire* in this volume) played a considerable role. To that question, and to the Jacobins, who undoubtedly took such steps, we shall return.

As Gauchet suggests, tension between liberty and authority in the French Revolution was exacerbated by the doctrine of representation, which won acceptance as the Third Estate transformed itself into a National Assembly. The early and decisive steps in the articulation of this doctrine by Abbé Sieyès and others have been traced in an important article by Keith Baker at the Chicago symposium; in the *Critical Dictionary*, the doctrine is considered in articles on sovereignty, liberty, constitution, and elections.²² Once elected, deputies were deemed to be representatives of the entire nation rather than their specific constituents. They were to be entirely free of such constraints as the imperative mandates that had bound deputies to the Estates General in the past or new mechanisms such as referenda. Deputies were free to pass any laws they deemed necessary, because the Constituent Assembly interposed no supreme court or other institution to interpret the constitutionality of particular laws. From the beginning of the revolution, the legislature, as the embodiment of popular sovereignty, was the sole interpreter of its own will. When citizens in the streets and clubs reappropriated popular sovereignty in 1792, they recognized no limitation either. Thus the Constituent Assembly laid the foundation for the abuse of power that became manifest in the Terror.

Is this the best light in which to judge 1789? The Assembly, after all, provided for biennial elections at every level of government, administration, and judicial power. Patrice Gueniffey—author of a pioneering doctoral thesis (under Furet's direction) on suffrage, electoral participation, and elections from 1790–1792—offers a critique of these elections, but the evidence of his own dissertation can be read somewhat less bleakly.²³ To be sure, electoral procedures were inordinately laborious and time-consuming, which helped cause high rates of abstention. But the assemblies were generally open rather than rigged, even (with a few exceptions like Paris) after August 1792.²⁴ The lack of official mechanisms for declaring or promoting candidacies made the process cumbersome and soon provided the Jacobin clubs with a useful opening, but from the beginning it was at least a simple matter to vote out an incumbent or to reward him with reelection. By the same token, going back a moment, the self-denying ordinance of the Constituent Assembly in 1791—interpreted implausibly if not ludicrously in the *Critical Dictionary* as a self-serving ploy by Robespierre to clear the field for the ascendancy of the Jacobins²⁵—established the credibility of an Assembly committed to a healthy

²² Baker, "Representation," in Baker, ed., *Political Culture of the Old Regime*. See also his article on sovereignty in the *Critical Dictionary*.

²³ Gueniffey, "La Révolution française et les Elections: Suffrage, participation et élections pendant la période constitutionnelle (1790–1792)" (Thèse pour le Doctorat de l'Ecole des Hautes Etudes en Sciences Sociales, 1989); Furet and Ozouf, *Critical Dictionary*, 33–44. Among other studies of elections, see Malcolm Crook, "Les Français devant le vote: Participation et pratique électorale à l'époque de la Révolution," in *Les Pratiques politiques en province*; and Melvin Edelstein, "L'Apprentissage de la Citoyenneté: Participation électorale des campagnards et citadins (1789–93)," *L'Image de la Révolution française*, 1: 15–25. But contrast Edmund Morgan, *Inventing the People: the Rise of Popular Sovereignty in England and America* (New York, 1988), chap. 8.

²⁴ On the elections of 1792, see Gueniffey, "La Révolution française et les Elections," chap. 15; and Alison Patrick, *The Men of the First Republic: Political Alignments in the National Convention of 1792* (Baltimore, Md., 1972), chap. 6.

²⁵ Furet and Ozouf, *Critical Dictionary*, 305.

kind of political freedom in which the will of the voters (and not simply an abstract people) was indeed supreme. It was on the terrain of political will that liberty failed, not on the level of institutions or ideologies.

What was indisputably subversive to liberal ideals was the (Rousseauist) vision of a unitary national will, which undermined the instinct toward pluralism and the possibility of legitimate, organized opposition (see the articles on liberty, equality, and public spirit, among others). For the Assembly, as for the infant American republic of 1789, political party was held to be nothing more than faction or cabal. The Jacobin Club, after the schism of 1791 and the departure of the Feuillants, began to act like a party but claimed that it was simply articulating the best interests of nation as a whole. Thus began the fatal competition to speak for "the will of the people." Whereas the Feuillants believed in September 1791 that the revolution was over, the Jacobins considered the revolution incomplete, regarding it as threatened with subversion and in need of defense by revolutionary means. The Jacobins would achieve unity by partition and exclusion.

This critique of 1789—of the imbalance in the Declaration of the Rights of Man, of the Assembly's unlimited concept of popular sovereignty and representation, of its unitary tropism—is suggestive, but against what should it be measured? From time to time, the authors obliquely invoke the yardstick of political pluralism, healthy interest politics, and the notion of anterior liberties not subject to erosion by legislative fiat. They clearly have in mind the Federalist Papers and the U.S. Bill of Rights, but without regard to the subsequent political and constitutional history of those distinctive ideals. In fact, their perspective involves a peculiar kind of formalism. Neither the U.S. Constitution nor the liberal traditions of the American polity could contain the rebellious political will of southerners in the 1850s. The level of upheaval and violence that ensued when the Constitution failed and the Union was sundered in 1861 was at least as great as that suffered by France in the 1790s. Moreover, even after the Constitution was restored (and morally redeemed with the 13th and 14th Amendments), the ill-defined triangular arena that existed between the Constitution, the state and federal legislatures, and the powers of judicial review proved to be extremely feeble in protecting the rights and liberties of individuals against the sheer force of political will and demagoguery. For black citizens in the South, to cite only the most obvious example, the Constitution and Bill of Rights—free from the defects described by Gauchet and Ozouf—did not preclude a reign of terror that lasted eighty years.

ACCORDING TO THE AUTHORS of the *Critical Dictionary*, the Constituent Assembly did not compromise with the old order but pushed through a supremely radical blueprint for the new regime right from the start. This argument is aimed directly at the conventional historiography of the Aulard-Mathiez-Lefebvre tradition, which held that the revolution of 1789 was incomplete precisely because it accepted a number of fundamental compromises: the Assembly backed away from republicanism and democracy by giving the king substantial executive powers, by limiting the franchise and eligibility for office according to fiscal criteria, and by failing to abolish the lion's share of seigneurial dues outright, instead making them subject only to extremely expensive and difficult redemption payments. To Furet and Ran Halévi, however, this appearance of compromise is deceptive and essentially cosmetic. The great rupture of August 4 did effectively destroy feudalism and

social hierarchy, even if seigneurial dues lingered under the guise of property rights. And this event was almost immediately followed by the destruction of the monarchical state in the autumn of 1789—by a “découronnement inédit,” the “salvaging of the king within a completely republican system,” after the Assembly rejected the veritable compromise proposed by the Monarchiens.²⁶ This notion is echoed as an article of faith in the essay on Robespierre: “One after another all the party leaders had attempted to control the course of events by becoming advisers of a phantom monarch whose authority they themselves had destroyed . . . While his adversaries gazed upward toward an empty space, [Robespierre] cast his eyes downward, toward the people, which filled the entire stage.”²⁷ The monarchy, it would seem, was no longer an actor with any force after the autumn of 1789.

A somewhat wistful article by Ran Halévi on the Monarchiens—the first group of conservative revolutionaries to be bypassed or disillusioned by the revolution’s momentum—seems to cast these men as the truly wise *constituants*. Writing in August 1789, their spokesman Jean-Joseph Mounier (who had led the Third Estate into the supreme revolutionary act of establishing the National Assembly in June) argued that only by maintaining the king’s prerogatives could the nation avoid a new kind of despotism from the untrammelled power of the legislature or the people. The Monarchiens attempted “to reconcile the rights of princes with the rights of man,” to maintain a degree of continuity between the Old Regime and the revolution. Accordingly, they rallied around two propositions that were eventually voted down by the Assembly decisively: an absolute royal veto and an upper legislative house of peers. The classic debate on this question between Sieyès and Mounier also figures in the conclusion to the first volume of R. R. Palmer’s *Age of the Democratic Revolution*. While both accounts are impressive, for tone and thrust Palmer’s seems sounder. Probing the arguments on all sides critically, and bringing in apt references to American perspectives, Palmer concluded unambiguously that Mounier was “tragically mistaken,” his position “untenable.”²⁸ Halévi understands the reasons for Sieyès’s victory and the element of wishful thinking in the Monarchiens’ appeal to historical continuity, yet he still leaves the impression that, by forgoing this compromise, the Assembly left the nation defenseless against tyranny in the unbounded sovereign space it thereby created.

That the Assembly then went on to circumscribe this sovereignty by such devices as a propertied franchise (*cens*), a suspensive veto for the king, and substantial appointive powers for the royal executive are mere details in this view. Gueniffey suggests that the primary franchise was, in effect, “a prelude to universal [male] suffrage” because the threshold was set so low. Unsurprisingly, the fall of that barrier did not demonstrably produce an influx of new voters in 1792, while the most arguably undemocratic element of the system—the indirect election of deputies by electors—survived at that time. The censitary franchise was important mainly as a talking point for radicals, for the *surenchère révolutionnaire* (a favorite term of Furet’s), who could attack it for partisan ends as a contradiction in the new regime’s ideology.²⁹ But, even if it is true that most citizens who wished to vote in

²⁶ This argument is summarized in Furet and Halévi, “L’Année 1789,” 3–24, and in a more expansive form in the introduction to Furet and Halévi, eds., *Orateurs de la Révolution française*, Vol. 1: *Les Constituants* (Paris, 1989). See also Halévi, “La Révolution constituante: Les Ambiguïtés politiques,” in Lucas, ed., *Political Culture of the Revolution*.

²⁷ Furet and Ozouf, *Critical Dictionary*, 301.

²⁸ R. R. Palmer, *The Age of the Democratic Revolution*, Vol. 1: *The Challenge* (Princeton, N.J., 1959), 499; Furet and Ozouf, *Critical Dictionary*, 370–78.

²⁹ Furet and Ozouf, *Critical Dictionary*, 573–78.

local elections or for electors could do so, many were not eligible to be elected or to serve in the crucial role of electors. Indeed, as the authors are at pains to suggest, the *constituants* tended to regard voting as a kind of civic function to which qualifications could rightly be attached, rather than as a simple right. This was bound to be a contentious issue.

The case for the monarchy's complete loss of theoretical sovereignty in 1789 is beyond dispute. But that leaves in question the practical consequences of the substantial executive powers vested in the king by the constitution, which indeed surpassed those of his English counterpart: a suspensive veto whose writ could run at least four years and the power to appoint ministers as well as generals entirely of his choosing. A recent history of the Legislative Assembly (1791–1792) suggests that this sovereign body attempted consistently to respect the independent exercise of the king's prerogatives, no matter how much grief it gave them. The new deputies held faithfully to the letter and spirit of the constitution of 1791 until the last possible moment, despite the king's uncooperative, disdainful, and ultimately treasonous behavior.³⁰ Even though Furet's article on Louis XVI is a beautifully written and fair-minded exposition, it is virtually the last comment in the *Critical Dictionary* about the role of the king and his supporters in the course of events. Furet has a keen sense of political will when it emanates from the Jacobins but not when it comes from royalists. It is one thing to view royalism or the refractory clergy as outgrowths of decisions taken by the revolutionaries and not as things external to the revolution itself, quite another to minimize the need of the revolutionaries to confront the threatening pressures that those elements exerted.

Halévi's assertion that the Feuillants "were the Revolution's last moderates" is not helpful either.³¹ It works against a sense of the revolution's fluidity and makes it harder to understand why the "center" had a persistently difficult time during the entire decade yet finally prevailed (albeit in a very limited sense) after the Coup of 18 Brumaire. Interestingly, there is no article in the volume on "The Plain," perhaps because the authors are not comfortable handling such relatively non-ideological deputies. Indeed, the volume's use of the term "moderate" can be downright bizarre, as in Gueniffey's observation about the elections of the Year IV, which were decided in part by the outgoing Convention itself and in part by the electors: "The electorate confirmed these initial moderate results, for fully half the deputies in the newly elected third of the Assembly were out-and-out monarchists[!]"³² In any case, the moderate temperament could be expected to shift with circumstances; it is a decidedly relative term. Even after the Jacobins prevailed in August 1792, for example, moderates contested the mayoralty of Paris and the protracted elections to the Paris municipality.³³ In the real world, there are always new battles to be fought, alliances to be forged, and tactics to be chosen, including a temporary retreat into private life.

EARLY ON, THE REVOLUTIONARIES OF 1789 made the extravagant claim that the French Revolution had created a complete break with a bankrupt past, believing that "there was nothing worth saving and no guidance for the coming unprece-

³⁰ C. J. Mitchell, *The French Legislative Assembly of 1791* (New York, 1988), esp. part 4.

³¹ Furet and Ozouf, *Critical Dictionary*, 350.

³² Furet and Ozouf, *Critical Dictionary*, 42.

³³ Maurice Genty, *L'Apprentissage de la citoyenneté: Paris 1789–1795* (Paris, 1987), 193–200.

dented adventure." In contrast, it could be said that earlier revolutions in England and the American colonies had sought at least in part to purify venerable ideals or traditional institutions. Furet has always been fascinated by the revolutionaries' astounding contention that they were effecting a complete break with the past. In his earlier writings, he warned against taking this notion at face value, as orthodox historians were prone to do. At the time, Furet was interested in the revolution's origins and was intent on suggesting its roots in the Old Regime itself—the tensions among the Old Regime's competing elites, most of which derived from the dysfunctions in the social and political system created by the monarchy. Then, in 1789, he observed, the monarchy itself designed the innovative democratic methods for electing the Estates General that the revolutionaries later adapted.³⁴ Attacking the regnant bourgeois-noble conflict theory of the revolution's origins, Furet was arguing that the crisis of the 1780s was susceptible to other outcomes and did not have to produce the French Revolution as we know it. In Phase Two of his project, this question takes on a very different inflection.

Now the crucial point is that the Assembly *believed* it had broken completely with a bankrupt past and inaugurated a new era for humanity in which the French nation would be "regenerated." To paraphrase (and simplify) the argument: the revolutionaries intended to transform the mores of society and to align them with the institutions of the new, rational polity they were creating. The state and its new system of laws would lead civil society to slough off the constraints of tradition and prejudice that had imprisoned it. For Furet and Ozouf, this regenerative fervor, present almost from the beginning, was the most fatal element of revolutionary ideology and practice. Fatal because, presumably, the traditionalism of most French men and women—their respect for habit and custom, their distrust of any government—assured that a meddlesome program of "regeneration" was bound to produce serious conflict between the revolutionary elites and the inertial mass of society.

Placing this problematic notion of regeneration at the core of the revolutionary project is one of Furet and Ozouf's most characteristic strategies. But here the thesis of latent illiberalism—implicit in the volume's discussion of liberty and authority in 1789—verges on a sense of illiberal predestination, which becomes almost palpable in the articles on regeneration, Jacobinism, Robespierre, public spirit, and the Terror. "Only Rousseau abandoned all consideration of what was possible," writes Ozouf, "and that abandonment is one of the reasons why the Revolution was all his from the beginning. It was the sudden, unprecedented character of the revolutionary rupture that blazed a trail for the idea of regeneration."³⁵ "Alongside 'regeneration,'" she continues, "the word 'reform' soon lost its luster." (A contention that I would seriously question.) In her article on regeneration, largely a free-ranging history of the idea, Ozouf observes that this utopian vision had its optimistic liberal variant—the implausible notion entertained by some revolutionaries of an almost spontaneous regeneration of mores in 1789 itself—but that its darker side was by far the dominant motif: the need to deal harshly with supporters of the past, to undo the past by proscribing, eliminating, purging. "And once the purge was accomplished, it then became necessary, in a

³⁴ François Furet, "La Monarchie et le règlement électoral de 1789"; and Ran Halévi, "La Monarchie et les élections: Position des problèmes," both in Baker, ed., *Political Culture of the Old Regime*.

³⁵ Furet and Ozouf, *Critical Dictionary*, 781–82.

second phase, to surround each individual with a web of myths, rituals, and beliefs as restrictive as possible and as confining as what had gone before."³⁶

This rather nebulous discussion is then illustrated with reference to revolutionary plans for education. Predictably, the author focuses on the LePelletier Plan, whose axiom was the common Jacobin notion of the need for all French children to experience an *éducation commune*, but whose novelty lay in the requirement that parents surrender their offspring to the ministrations of highly regimented state boarding schools. In fact, this Spartan fantasy sidetracked the Convention for little more than the one day in which it was debated and adopted (for reasons having to do with political expediency, I believe), only to be indefinitely suspended on the morrow, never to be heard of again except from historians.³⁷ Distorting this discussion further, Ozouf's brief consideration of the subsequent history of revolutionary policy on education omits the most considered and sustained attempt to implement free public primary schooling: the Lakanal Law of November 1794—a landmark in both theory and practice, which aborted after a year of dedicated effort when the hyperinflation of 1795–1796 rendered nil the fixed state salary for teachers that was the project's keystone.³⁸ But, outside the article on *assignats*, this hyperinflation, a torrent that raged across the scene with enormous impact, leaves scant traces in the *Critical Dictionary*.

Selectively, at least, the volume is exemplary at incorporating the Thermidorian–Directory years into its ken, and Ozouf correctly observes that the notion of regeneration was far from moribund during the Directory years. By coincidence, we have both written on the most contentious manifestation of this impulse: the Directory's attempt to impose the extremely unpopular ten-day republican calendar instead of quietly letting it lapse. Ozouf frames this issue in maximalist fashion with the break-with-the-past theme: the real issue being forced by the republican calendar was the need to coerce the people into recognizing that 1789 divided time itself, meaning history, into two totally distinct eras. My own view is that this misguided policy was an episode in the disastrous Voltairian struggle with the church that had erupted, without anticipation, in 1790.³⁹ Education, too, could be construed as a battleground for this impulse, whatever its roots. But, again, in the article on regeneration (the only place in the *Critical Dictionary* where the fundamental question of education is considered at any length), Ozouf's truncated discussion emphasizes exclusively the government's coercive proposals and neglects the variety of other approaches competing for approval to reverse the benign neglect of the Daunou Law of 1795 and revive the state's commitment to public primary education. In the end, it is true, the Directory on its own authority implemented coercive measures against Catholic schoolmasters—which cost the state no money but lots of good will—while the more constructive possibilities, such as a progressive local school tax, were bogged down in protracted parliamentary debate until Brumaire rendered them moot.⁴⁰ But that should not matter, since

³⁶ Furet and Ozouf, *Critical Dictionary*, 785.

³⁷ The text of the LePelletier Plan is published and analyzed in Branislav Baczczo, *Une Education pour la démocratie: Textes et projets de l'époque révolutionnaire* (Paris, 1982). Compare R. R. Palmer, *The Improvement of Humanity: Education and the French Revolution* (Princeton, N.J., 1985), chap. 4.

³⁸ This argument will be developed in a chapter on primary schooling in my forthcoming book, *The New Regime: The Transformation of the French Civic Order, 1789–1820s*.

³⁹ Mona Ozouf, "Passé, présent, avenir à travers les textes administratifs de l'époque révolutionnaire," in *L'Ecole de la France: Essais sur la Révolution, l'utopie, et l'enseignement* (Paris, 1984); and Isser Woloch, "Republican Institutions, 1797–1799," in Lucas, ed., *Political Culture of the Revolution*.

⁴⁰ See Maurice Gontard, *L'Enseignement primaire en France de la Révolution à la loi Guizot, 1789–1833* (Paris, 1959), 171–83; and Palmer, *Improvement of Humanity*, 257–78.

results as such are rarely the criteria for emphasis in Ozouf's sequence of articles on the revolution's key ideas.

Even during the Jacobin Convention, the French Revolution was an amalgam of pragmatism and fanaticism, of reformism and utopianism. On one level, the authors of the *Critical Dictionary* recognize this duality or ambiguity; indeed, it sometimes functions as their inspiration, generating the question, for example, of "how liberal principles could have coexisted with a coercive pedagogy during the Revolution." But the freewheeling essay form of the *Critical Dictionary* does not put a premium on addressing such questions in a systematic or balanced manner. In the final analysis, the authors can rarely resist exaggerating their argument for dramatic effect: "Having granted the individual his full rights, the Revolution had to find a way to bring him into step with the community, to couple *absolute* liberty with *total* obedience."⁴¹ The reader can scarcely be surprised when Ozouf's seductive argument concludes all too predictably on this false note: "We touch here on the heart of the relation between the French Revolution and totalitarianism, both of which seek to create a new man."⁴² Are the objectives and practices of Nazism, Soviet Communism, or Maoism, not to mention the scale of their violence and the techniques of their oppression, really relevant to understanding the French Revolution? Such speculative theorizing will not, I think, pass the test of common sense.

THE FIFTEEN-PAGE ARTICLE ON ROBESPIERRE must be one touchstone of the *Critical Dictionary*. Throughout the volume, the authors reflexively assume Robespierre to be the avatar of the revolution's potential for fanaticism, exclusion, and tyranny. It is Patrice Gueniffey's task to draw together the case for this responsibility. In one sense, the volume's focused animus toward Robespierre is fair play, since his image has always loomed above most others. The two great activist-historians of the democratic left, Louis Blanc and Jean Jaurès, held Robespierre in esteem despite some reservations, but the trouble really began with Albert Mathiez, when the Incorruptible's zealous protagonists literally tried to place Robespierre on a pedestal. (With the erection of a statue of Robespierre in his birthplace, Arras, in the 1930s, they finally succeeded, but recently the statue has reportedly been banished to the basement of the city hall.) While it may have been plausible in the 1920s and 1930s for the organization of French revolutionary scholars to call itself La Société des Etudes Robespierristes, the continuing identification seems singularly provocative and inappropriate. Yet, just last year, Michel Vovelle explained in a public address (vaguely and rather half-heartedly, it must be said), "Why We Are Still Robespierrists."⁴³

Clearly, there is a continuing need to approach Robespierre critically, although balanced evaluations without adulation have long been available. In the American liberal tradition, R. R. Palmer's classic study of the Committee of Public Safety is a model of how to deal with Robespierre, although Palmer is concerned only with

⁴¹ Furet and Ozouf, *Critical Dictionary*, 790, my emphasis.

⁴² Furet and Ozouf, *Critical Dictionary*, 789.

⁴³ Albert Mathiez, "Pourquoi nous sommes Robespierristes," (1920), reprinted in his *Etudes sur Robespierre* (Paris, 1958); Michel Vovelle, "Pourquoi nous sommes encore Robespierristes," *Annales historiques de la Révolution française*, 274 (1988): 498–506. Mathiez never had a chance to write a full-scale study of Robespierre, but the admiring view is set forth in George Rudé, *Robespierre: Portrait of a Revolutionary Democrat* (New York, 1975).

Robespierre's exercise of power in the Year II, not with his rise to power or his influence on the course of the revolution before 1793.⁴⁴ A balanced view of the kind devoted to the comte de Mirabeau or Georges Danton, however, is not the *Critical Dictionary's* purpose. Rather, in an essay calibrated with Furet's article on Jacobinism, we are given a devastating psychological-political portrait that envelops the man and, with him, the entire Jacobin presence in the revolution.

Gueniffey's imaginative essay offers fresh insights, but the effect is marred by an unrelenting if subterranean hostility, by the suffocating unwholesomeness of purpose that is ascribed to Robespierre. No turn in Robespierre's career is left unstoned, and nothing really answers Gueniffey's own question of why such a verbose rhetorician and unappealingly self-righteous figure should have stirred such enthusiasm in the Paris Jacobin Club, in the sections, and in the provincial clubs. Gueniffey can place even the most politic act in a negative light. I have already mentioned his dubious interpretation of the self-denying ordinance successfully advocated by Robespierre in the Constituent Assembly, and his comments on Robespierre's maneuvers against sanguinary deputies-on-mission who had manifestly abused their authority are equally perverse.⁴⁵

Gueniffey reasonably allows that, "because of . . . continual changes, any attempt to present a coherent portrait of Robespierist ideology is doomed to failure." But if his ideology was not consistent, what was? Surely, he was not a mere opportunist, still less a pragmatist. In the first place, like Furet and Ozouf, Gueniffey finds the concept of regeneration at the heart of his subject. Robespierre, he contends, had "eliminated in himself all distinction between private and public, between love of self and love of country; he had completed the cycle of 'regeneration' that would become the center of his politics."⁴⁶ Secondly, Gueniffey makes a subtle case for a special kind of opportunism: "Robespierre's genius was to be always in tune with the times. Had he allowed himself to be hemmed in by a coherent and finished system, he would have been obliged to give up speaking in the name of the people . . . All definite ideas concerning laws, institutions, or the form of the government were overshadowed by the strategic imperative."⁴⁷ Up to a point, this is a plausible judgment, but in fact Robespierre was not always in tune with the times at critical moments, for instance, his risky exposure in opposing the war fever of late 1791–early 1792, or his understandably difficult time in convincing himself and then the Convention to liquidate Danton.

On every page, Gueniffey attempts to forge a new key that will unlock the mystery. "[D]ead to himself," he observes at one point, "Robespierre was all action, and in his case one might almost have 'deduced his existence from his acts,' as Kierkegaard said of God." Yet later, noting Robespierre's self-appointed role as the National Assembly's conscience or censor, he must reverse gears: "Anchored in the world of principles, Robespierre did not act. In him immobility became action, and he stood as a fixed point in the tumult of the Revolution."⁴⁸ There is such a thing

⁴⁴ R. R. Palmer, *Twelve Who Ruled: The Year of the Terror in the French Revolution* (1941; rpt. edn., Princeton, N.J., 1989), esp. chaps. 11–12. This peerless book was finally translated into French in 1989, under the title *Le Gouvernement de la Terreur: L'Année du Comité de Salut public*. The publisher, Armand Colin, requested François Furet to write the foreword, and it is to his great credit that his own views on the subject, which are manifestly at variance with Palmer's, did not intrude in the least. See also Norman Hampson, *The Life and Opinions of Maximilien Robespierre* (New York, 1974).

⁴⁵ Furet and Ozouf, *Critical Dictionary*, 309.

⁴⁶ Furet and Ozouf, *Critical Dictionary*, 299.

⁴⁷ Furet and Ozouf, *Critical Dictionary*, 307.

⁴⁸ Furet and Ozouf, *Critical Dictionary*, 300–301.

as being too clever, and one must eventually call a halt: was Robespierre “ideology incarnate” or “all action”; a self-adjusting machine or a fixed point? Gueniffey is too entangled in the dark poetics of Robespierre’s life to serve as a useful guide. The brio with which he tosses off such remarks comes at the expense of coherence. In the end, the enigma, Robespierre’s “strange power of incarnation,” remains an enigma in these pages perhaps because context and events, among them sans-culotte politics and egalitarian aspirations, are missing from the account: all the things to which Robespierre—willful, inflexible, and peculiar as he may have been—was responding.

The great difference between more balanced readings such as Palmer’s and the approach of Furet and his colleagues, I believe, is traceable to the French blanket denial that circumstances shaped the revolution or that “revolutionary necessity” could ever justify acts that were inexcusable by ordinary standards of liberal principle or morality. The critique of this concept runs through most of the articles dealing with the Year II or with historiography. Furet rightly observes that, in the nineteenth century, a common strategy of interpretation was to relate the Terror to circumstances allegedly *external* to the revolution or, alternately, to blame it on the pressures generated by irresponsible plebeian demands. Aulard in particular is taken to task for his belief that the historical process “was constantly influenced by adverse circumstances, so that the real Revolution, hampered by unfinished business, conflict, and setbacks, only partially fulfilled the promises of the ideal Revolution.”⁴⁹ In his own way, Aulard was indeed concerned with the gap between intentions and results, but, unlike most of the philosophical historians whom Furet admires, he did not see this as an enigma, let alone a profoundly disturbing one.

Aulard was the chief editor of the massive, twenty-eight volume *Recueil des Actes du Comité de Salut public et correspondance des représentants en mission* (1889–1951), which receives only one passing line in Furet’s essay. It was in editing the papers of the Committee of Public Safety and the Convention’s deputies in the field that Aulard’s positivist program actually made for good history by excavating the chronology and detail of the crisis. It is true, as Cochin pointed out long ago, that this was, in effect, an official Montagnard history of the revolution,⁵⁰ but it is no less essential for a critical history of the period. Aulard believed that to study the revolutionary government without reference to the counterrevolution and other “circumstances” would be like excluding from view one of the two parties to a boxing match or duel: the other party would simply appear to be a madman.⁵¹ The utility of Aulard’s scholarship is apparent in Palmer’s *Twelve Who Ruled*, which remains the best history of the Terror and the revolutionary government of the Year II in any language.⁵² Written entirely from published sources, the book depends fundamentally on the mass of material that Aulard’s *Recueil* placed at the disposal of historians. Palmer captured the fluidity of the situation, the spectrum of motives, behavior, and results. In effect, he explored the concept of “revolutionary necessity,” neither swallowing it whole nor rejecting it categorically.

From time to time, Furet joins the debate on Aulard’s level by arguing that the most murderous policies of the revolutionary government were not necessary by any criterion. The repression in Lyons and in the Vendée, he contends, reached its peak months *after* the Republic had prevailed militarily against any actual threat.

⁴⁹ Furet and Ozouf, *Critical Dictionary*, 887.

⁵⁰ Augustin Cochin, *La Crise de l’histoire révolutionnaire: Taine et M. Aulard* (Paris, 1910).

⁵¹ See, for example, Alphonse Aulard, *Taine, Historien de la Révolution française* (Paris, 1907).

⁵² See note 44.

He also underscores the disturbing fact that the judicial Terror intensified at the moment of the Republic's great military victory at Fleurus in June 1794. With Ozouf, Furet denies the pragmatists' claim that the Terror was an arsenal for victory; rather, it confused the war effort with an ambition for regeneration and a unitary national will.⁵³ Putting it another way, he agrees with Benjamin Constant that, far from binding the French people to the Republic, the revolutionary government alienated them. These points deserve careful consideration, particularly in regard to the nature and impact of civil and external war. (A lone article on the army by Alan Forrest is virtually ghettoized in the volume.) War brought unprecedented mobilization with political ramifications that were indeed linked to the Terror, and that did not abate with the victory of Fleurus or the rout of the "Catholic and Royalist Army" in the Vendée. Both were victories in different kinds of wars that promised to drag on; the panic was over, but the need to prevail remained.

The will to win those wars is commonly attributed to the Montagnards, along with much else. In a nuanced, debunking article on the Mountain and "the traits that earned it its prestige among historians," Ozouf deems their interest in establishing the revolution's egalitarian potential in such areas as social welfare to be relatively unimportant. Far more central was their dedication to a warped ideal of national unity—a unity promoted through relentless purges, including the prosecution of the "federalist" heresy. (Her essay on federalism—echoing the recent studies of Bill Edmonds and Alan Forrest—exposes the Montagnard version of a counterrevolutionary "federalism" as a fabrication entirely lacking credibility. Once the rebellion against local Jacobin domination in Lyon, Marseilles, and Toulon climaxed, however, the Convention faced an actual revolt in such urban centers against its writ.⁵⁴) Ozouf's assessment of the Montagnards reiterates Furet's view of Jacobinism as a "temple of orthodoxy," "a machine for producing unanimity" in the Jacobin Club's own ranks, in the Convention, and in revolutionary opinion at large. Not only was the Paris Jacobin Club the most adept and powerful of the oligarchies that thrived in the revolution's political culture, it actually regarded itself as "a proxy for the popular will." In the end, the Montagnard–Jacobin amalgam, as the self-proclaimed voice of the people, usurped national sovereignty and became a kind of ruling party. But how did this come about?

In one passage, Ozouf presents the standard case for the Mountain's ascendancy: starting in March 1793, the Montagnards began to command increasing support among unaligned deputies who

decided to vote with the Mountain in support of revolutionary measures. Barère was both an example and the theorist of this tendency, in which, according to Baudot, a sense of necessity played a decisive role. Many deputies were convinced that when a 'revolutionary' measure was passed—even one as detestable and detested as the Convention's acquiescence in its own mutilation on May 31—the consequences had to be accepted: acceptance of what seemed inevitable was the key ingredient of the Mountain's triumph.⁵⁵

⁵³ See the articles on the Terror, revolutionary government, Jacobinism, and the Vendée. Also Mona Ozouf, "Guerre et Terreur dans le discours révolutionnaire," in *L'Ecole de la France*.

⁵⁴ Bill Edmonds, "Federalism and the Urban Revolt in France in 1793," *Journal of Modern History*, 55 (1983): 22–53; Alan Forrest, "Federalism," in Lucas, ed., *Political Culture of the Revolution*; and Malcolm Crook, "Federalism and the French Revolution: The Revolt in Toulon in 1793," *History*, 65 (1980).

⁵⁵ Furet and Ozouf, *Critical Dictionary*, 385.

This view is cited, however, merely to be discarded as a feeble rationalization of what the Mountain truly represented. We come closer to Ozouf's sense of the matter when she writes with uncharacteristically bald sarcasm: "The Montagnard episode thus gave birth to a new type of hero: the ruler who rules despotically in the name of liberty, the general who makes war while hating it, the Terrorist who uses the guillotine without loving it, all united by their acceptance of necessity. Thus, as Esquiros later put it, 'the regrets and complaints engendered by the Terror must fall before these words, as sharp and unyielding as an axe: it was necessary.'"⁵⁶ Expressed this way, one can only resist the notion of "revolutionary necessity," and the *Critical Dictionary's* skepticism seems salutary. But what about a full hearing for the other side? Ozouf's passing allusion to Bertrand Barère merits amplification.

Even within the terms of their own brief, the editors should have assigned an essay on Barère. In its absence, we can attempt to fill in the picture ourselves with the aid of Leo Gershoy's scrupulous study, perhaps the best English-language biography of any revolutionary figure.⁵⁷ This "reluctant terrorist" was not the cynical or weak-willed opportunist that his detractors later claimed. Exploring Barère's prominent and often agonizing role in most of the key episodes before and during the Terror would give some depth to this problematic notion of "revolutionary necessity." Barère's hard choices included his alignment with the Jacobins after a brief flirtation in July 1791 with the Feuillants, who wished to make him their president; his intervention against Pierre Vergniaud's call for an "appeal to the people" on the king's death sentence; his role as *rapporteur* justifying the expulsion of the Girondins, which he had tried mightily to prevent; and his bid to the Convention in September 1793 that it endorse the Parisian demand to place terror on the order of the day. To pass over Barère with one sentence, while dissecting Jean-Paul Marat and Robespierre in detail, is to assure that the less fanatical proponents of "revolutionary necessity" do not receive a fair hearing. Furet is not oblivious to the notion that the Convention, influenced by people like Barère, sought to organize and circumscribe the Terror, by alternately placating and undercutting the sans-culottes. But this classic issue is not taken very seriously. Perhaps alongside the concepts that are explored at length in the *Critical Dictionary*, the editors ought to have investigated the uses of the term "anarchy." Fear of chaos and popular violence probably ran as deeply among the *conventionnels* as the obsessive dread of aristocratic plots and counterrevolutionary conspiracies.

IT IS USUALLY BAD FORM for a reviewer to criticize the book that an author failed to write. In the case of a volume that presents itself with the encyclopedic scope and interpretive ambitions of the *Critical Dictionary*, however, the question of selection must be a legitimate concern. The editors' criteria for selection seem calculated to create a centripetal field of discourse rather than one that opens the subject out. As a result, fundamental topics that could yield insight into the revolutionary experience are by-passed: agrarian policy, fiscal policy, including forced loans and progressive taxation (the article on taxation is about the problems of Old Regime fiscality); slavery in the colonies; the concept of law; and the revolution's criminal codes, to name only a few. Burke is reverently treated, while Thomas Paine is

⁵⁶ Furet and Ozouf, *Critical Dictionary*, 387.

⁵⁷ Leo Gershoy, *Bertrand Barère, A Reluctant Terrorist* (Princeton, N.J., 1962).

forgotten. Along with Barère, emblematic figures such as Merlin de Douai or LaRochefoucauld-Liancourt are neglected.

In the final analysis, of course, what counts is what the authors have chosen to discuss. A reviewer can only begin to suggest what is going on in a uniquely complex book of this kind. But the most original, provocative, and reiterated themes I believe to be the following: the revalorization of the philosophical historians of the nineteenth-century at the expense of the tradition of academic historiography; the notion that the National Assembly re-created the absolute authority of the monarchy in the new, unbounded space of popular sovereignty; the argument that, in its radicalism, the new polity of 1789–1791 did not really compromise with the old order; the relentless critique of the notion of “revolutionary necessity”; and, finally, the calamitous role ascribed to the revolutionaries’ purported project for the “regeneration” of French society. These themes create an underlying sense of the revolution’s latent illiberalism, and I expect that this is how the book will make its mark. Particularly for those interested in political philosophy and civics lessons, the French Revolution becomes a cautionary tale instead of an inspiration. The authors succeed admirably in reopening basic questions and introducing novel arguments, but, because of their narrow selectivity and penchant for exaggeration, these arguments often fall short. Effectively challenging so much conventional wisdom, the *Critical Dictionary* must itself be read critically.

Review Article
Medicine in Context:
A Review Essay of the History of Medicine

JUDITH WALZER LEAVITT

FIFTY YEARS AGO, Henry E. Sigerist, then director of the Johns Hopkins Institute of the History of Medicine, called for a new direction for the field, placing it and, indeed, medicine itself, within a broad social context. "We know much about the history of great medical discoveries but very little on whether they were applied or to whom they were applied," he observed. "In every medical action there are always two parties involved, the physician and the patient, or, in a broader sense, the medical corps and society. Medicine is nothing else than the manifold relations between these two groups. . . . [Medical h]istory thus becomes social history."¹ Although Sigerist could not have predicted all the changes to follow, his exhortation to move the focus of historical investigation from great physicians and their texts to include the context in which medicine was practiced foretold a new era in medical history. As exemplified by Sigerist's own writings, which often reflected his socialist interests, and those of historian Richard Shryock,² social concerns have been incorporated into medical history during the last half-century. Much of this social history of medicine comes from scholars who have had no formal connection to the field and who may not even know they are following Sigerist's model. The walls of the internalist discipline have been slowly falling away, and the context of health care practice broadly conceived has become of paramount interest to the wide variety of scholars currently writing on health.³

I am grateful to Vanessa Northington Gamble, Linda Gordon, Lewis Leavitt, and Ronald L. Numbers for their reading of an earlier version of this essay and for their helpful comments.

¹ Henry E. Sigerist, "The Social History of Medicine," read before the California Academy of Medicine in San Francisco, March 11, 1940, in *Henry E. Sigerist on the History of Medicine*, Felix Marti-Ibañez, ed. (New York, 1960), 25–33; quotations from 25, 26.

² Sigerist published widely in the history and sociology of medicine. See *Man and Medicine* (1932); *The Great Doctors* (1933); *American Medicine* (1934); *Socialized Medicine in the Soviet Union* (1937); *Medicine and Human Welfare* (1940); *Civilization and Disease* (1943); *Landmarks in the History of Hygiene* (1956); *Henry E. Sigerist on the Sociology of Medicine*, Milton I. Roemer and James M. MacKintosh, eds. (New York, 1960); *Henry E. Sigerist on the History of Medicine*; and *Henry E. Sigerist: Autobiographical Writings*, selected and translated by Nora Sigerist Beeson (Montreal, 1966). See also Richard H. Shryock, *Medicine and Society in America, 1660–1860* (Ithaca, N.Y., 1960); and *The Development of Modern Medicine: An Interpretation of the Social and Scientific Factors Involved* (1947; facs. edn., New York, 1969); and *Medicine in America: Historical Essays* (Baltimore, Md., 1966).

³ Some think the trend in the direction of social history has gone so far as to miss the medicine altogether. See, for example, Lloyd G. Stevenson, "A Second Opinion," *Bulletin of the History of Medicine*, 54 (1980): 134–40; and [Leonard Wilson], "Medical History without Medicine," *Journal of the History of Medicine*, 35 (1980): 5–7. For cogent reviews of recent historiography, see Gerald N. Grob, "The Social History of Medicine and Disease in America: Problems and Possibilities," *Journal of Social History*, 10 (1977): 391–409; Susan Reverby and David Rosner, "Beyond 'the Great Doctors,'" introduction to their edited collection, *Health Care in America: Essays in Social History* (Philadelphia, 1979), 3–16; and Ronald

Edward H. Beardsley, **A History of Neglect: Health Care for Blacks and Mill Workers in the Twentieth-Century South** (Knoxville: University of Tennessee Press, 1987).

Joan Jacobs Brumberg, **Fasting Girls: The Emergence of Anorexia Nervosa as a Modern Disease** (Cambridge, Mass.: Harvard University Press, 1988).

Ellen Dwyer, **Homes for the Mad: Life inside Two Nineteenth-Century Asylums** (New Brunswick, N.J.: Rutgers University Press, 1987).

Darlene Clark Hine, **Black Women in White: Racial Conflict and Cooperation in the Nursing Profession, 1890–1950** (Bloomington: Indiana University Press, 1989).

Irvine Loudon, **Medical Care and the General Practitioner, 1750–1850** (Oxford: Clarendon Press, Oxford University Press, 1986).

Hilary Marland, **Medicine and Society in Wakefield and Huddersfield, 1780–1870** (Cambridge: Cambridge University Press, 1987).

David McBride, **Integrating the City of Medicine: Blacks in Philadelphia Health Care, 1910–1965** (Philadelphia: Temple University Press, 1989).

Gloria Moldow, **Women Doctors in Gilded-Age Washington: Race, Gender, and Professionalization** (Urbana: University of Illinois Press, 1987).

Roy Porter and Dorothy Porter, **In Sickness and in Health: The British Experience** (New York: Basil Blackwell, 1989).

Susan M. Reverby, **Ordered to Care: The Dilemma of American Nursing, 1850–1945** (Cambridge: Cambridge University Press, 1987).

Charles Rosenberg, **The Care of Strangers: The Rise of America's Hospital System** (New York: Basic Books, 1987).

Laurel Thatcher Ulrich, **A Midwife's Tale: The Life of Martha Ballard, Based on Her Diary, 1785–1812** (New York: Alfred A. Knopf, 1990).

John Harley Warner, **The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885** (Cambridge, Mass.: Harvard University Press, 1986).

As medical history emerges as a subfield of social or cultural history, its historians are asking many questions different from those of their predecessors; to answer them, they are using different sources and approaches. In order to analyze recent themes in research, this essay concentrates on selected books about England and the United States in the late eighteenth, the nineteenth and twentieth centuries, all published within the last five years. The authors represent the variety of backgrounds now found in the history of medicine: historians trained in the field of the

L. Numbers, "The History of American Medicine: A Field in Ferment," *Reviews in American History*, 10 (December 1982): 245–63. The last essay is particularly helpful in tracing the pedigrees of medical historians and showing intellectual influence from the first generation through the most recent contributors.

history of medicine, physicians with no formal history training, historians without formal training in history of medicine or in medicine.⁴ Their very diversity indicates the amorphous nature of the field at the end of the twentieth century, more defined by subject matter than by particular approach, training, or departmental affiliation of practitioner. Yet these scholars and books agree (consciously or not) with what Sigerist argued: the history of medicine is the history of healers and sick people seen within the actual context of their interaction (social and intellectual), and one side without the other is only partial history.

Recent historical writing is wide ranging and diverse. I examine three areas that have occupied many scholars; for each, I have selected books that best represent the new scholarship.⁵ First, I discuss those historical works that have expanded our notions of healers and their institutions well beyond an elite group of physicians and their theories. Second, I examine books that, by attending to the patients' side of the story, illustrate the interactive process of health care and locate sources of power on both sides of the bed. Third, I turn to those scholars who use race, class, and gender to enhance our understanding of the range of healing practices and interactions. They show how social groups directed or were affected by historical changes and how status inequalities influenced health care. In short, the first two themes emphasize adding diversity and context to medical history, and the third probes the meanings of those additions. All the approaches veer away from interpreting medical history simply as a march of progress toward our late twentieth-century pinnacle of scientific acumen, an interpretation associated with the earlier emphasis on medical ideas and innovations. All consider the processes of change as important as the products of change. They reach beyond the great doctors and their writings to ordinary practitioners and their actions in particular contexts. Taken together, the books discussed here (some of which fit into more than one category) and the trends they represent reveal an area of scholarship that is active, vital, and exciting. They also indicate how far we have yet to travel toward a full understanding of health and sickness in the past.

⁴ Some excellent new work has been about earlier historical periods or about Africa and other areas of the world. See Ann Carmichael, *Plague and the Poor in Renaissance Florence* (Cambridge, 1986); Steven Feierman, "Struggles for Control: The Social Roots of Health and Healing in Modern Africa," *African Studies Review*, 28 (1985): 73–145; Richard J. Evans, *Death in Hamburg: Society and Politics in the Cholera Years, 1830–1910* (London, 1987); William Coleman, *Yellow Fever in the North: The Methods of Early Epidemiology* (Madison, Wis., 1987); and his *Death Is a Social Disease: Public Health and Political Economy in Early Industrial France* (Madison, 1982); James C. Riley, *The Eighteenth-Century Campaign to Avoid Disease* (New York, 1987); Matthew Ramsey, *Professional and Popular Medicine in France, 1770–1830* (Cambridge, 1988); Harold J. Cook, *The Decline of the Old Medical Regime in Stuart London* (Ithaca, N.Y., 1986); John Pickstone, *Medicine and Industrial Society: A History of Hospital Development in Manchester and Its Region, 1752–1946* (Manchester, 1985). Canadian studies are not covered in this review, although a vibrant historiography has developed in that country.

⁵ Other areas could have been chosen. For example, history and public policy has emerged as a growing area that deserves attention. See Rosemary Stevens, *In Sickness and in Wealth: American Hospitals in the Twentieth Century* (New York, 1989), an excellent study of hospitals with significant implications for current policy; and Allan Brandt, *No Magic Bullet: A Social History of Venereal Disease in the United States since 1880* (New York, 1985), which has importance for present-day AIDS policy. Also not covered in this review, but important to recent historical study, are works about medical schools and education. See Kenneth Ludmerer, *Learning to Heal: The Development of American Medical Education* (New York, 1985); William G. Rothstein, *American Medical Schools and the Practice of Medicine: A History* (New York, 1987); and Ronald L. Numbers, ed., *The Education of American Physicians: Historical Essays* (Berkeley, Calif., 1980). It should be noted that directions of scholarship in other parts of the world are in many ways similar to those discussed here.

HISTORIANS OF THE ANGLO-AMERICAN WORLD on both sides of the Atlantic have reached a consensus that a wide range of healers needs historical examination: in addition to elite physicians, the traditional focus of medical historical writing, there were non-elite regularly trained doctors, irregular practitioners of many varieties, nurses, folk healers, domestic practitioners, itinerants, midwives, apothecaries, clergy, and herbalists. Some of these groups are now receiving the attention they deserve; others await future investigators. Until we can trace their impact through time and place, we will have an incomplete and distorted picture of medical practice. Just how incomplete can be seen in Irvine Loudon's study of provincial general practitioners in eighteenth and nineteenth-century England, *Medical Care and the General Practitioner, 1750–1850*.⁶ Loudon, using a wide variety of sources including patients' accounts and novels, as well as practitioners' log books, parish records, and official publications, leads us to the conclusion that medical history focusing on London and on elite physicians and surgeons misses the health care experience of most Britons.

Loudon analyzes the forces that precipitated a rise in popularity of those who came to be called general practitioners, and he views the process both from the practitioners' interest in expansion and higher status and the people's desire for efficient, accessible, and affordable healers. He is especially persuasive when he considers what sick people wanted from their healers and indicates how their attitudes influenced professional developments and treatments. Loudon demonstrates the varied social backgrounds and education of the surgeon-apothecaries, those rank-and-file practitioners heretofore thought to have evolved from lowly tradesmen, and shows their broad appeal as orthodox practitioners of both physic and surgery. These provincial practitioners, trained through apprenticeships and with hospital experience, served the needs of ordinary people. The large majority of patients suffered the medical complaints of fevers, diarrhea, or consumption; or the surgical problems of leg ulcers, abscesses and inflammations, and minor accidents. It was the general practitioners' success in dealing with such common medical problems and minor injuries—their ability to diagnose ills, sell drugs, and set bones—that accounted for their increasing popularity.

This context allows readers to understand the “obstetrical revolution” spanning the eighteenth and nineteenth centuries, the increasing replacement of female midwives by medical men attending normal labor and delivery. Typically, this switch in attendants has been attributed to the innovation of the forceps; Loudon instead views the use of medical attendants in conjunction with the rise in the status and prosperity of the general practitioner. First, Loudon notes, male midwifery arose in England before the general adoption of the forceps. More important, it paralleled a growing prosperity among general practitioners, who sought to stabilize their progress by attracting new patients and expanding activities that fit in with general family attendance. Through the adoption of midwifery, practitioners hoped to create a clientele of loyal patients. Loudon is quick to remind us that, even during the late nineteenth-century heyday of general practice, female midwives continued to attend the majority of deliveries. By focusing on ordinary practitioners and their patients, and by viewing their interaction within a specific context while incorporating a variety of sources, Loudon well illustrates how a broad perspective alters and enriches our understanding of the complexity of health care practices in the past.

⁶ Irvine Loudon, *Medical Care and the General Practitioner, 1750–1850* (Oxford, 1986).

Another British historian, Hilary Marland, reveals a similar degree of complexity through a comparative community study, *Medicine and Society in Wakefield and Huddersfield, 1780–1870*.⁷ Marland also moves away from the geographic bias of a focus on London by concentrating on two Yorkshire towns. She, too, is interested in the interactions between a broadly conceived medical community and the public. Marland considers the effects of the economy on health care as she compares, in some detail, the medical services available in two communities, one of which, as a regional market, declined in the face of early nineteenth-century industrial expansion, the other of which took full advantage of industrial growth. In addition to analyzing the medical profession itself (especially general practitioners) and the institutional services available to the public through the government after the passage of the Poor Laws and through private charities, Marland pays particular attention to working-class self-help services (friendly societies) and alternative and sectarian forms of medical practice. The book is a study of medicine in context: it illustrates how attention to community setting can enhance our understanding of historical change in health care, and it identifies the wide range of factors that determined that change.

Marland's most significant contribution is her demonstration that social factors were as crucial to professional success as medical acumen. She shows through analysis of the records of people who supported medical charities that middle-class lay people influenced the direction and shape of medical services and professional development. The personal reputation that doctors gained through their interactions with lay volunteers in local medical charities and other civic activities often brought them lucrative practices. Thus the community behavior of doctors, as much as their medical performance, becomes, in Marland's hands, vital to an illumination of their professional history.

Crossing the Atlantic, we can see some of the same broadening of viewpoint among historians who study medical practice in the United States. John Harley Warner's *Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885*⁸ is an important work linking changes in scientific knowledge with particular medical experiences. Warner concludes, as would Sigerist, that both theory and practice must be studied to understand the complexity of forces influencing historical change. Warner compares regions through a close examination of the Boston Massachusetts General Hospital and the Cincinnati Commercial Hospital. After establishing the changes in theories of disease during the period, Warner connects these changes to practice by presenting physicians' actual therapeutic behavior in the two hospitals. He describes significant therapeutic distinctions between Boston and Cincinnati. Although the administration of heroic therapies of mercury and bloodletting gradually declined nationwide in this period of the nineteenth century, Warner demonstrates that patients in Cincinnati continued to be heavily dosed with calomel and more frequently bled than their Boston counterparts. To explain the differences, Warner looks at the professional status of physicians in both communities and examines the relationship between therapeutic practices and professional confidence. He concludes that the Cincinnati

⁷ Hilary Marland, *Medicine and Society in Wakefield and Huddersfield, 1780–1870* (Cambridge, 1987). There are many such community-based studies focused on the United States. See Judith Walzer Leavitt, *The Healthiest City: Milwaukee and the Politics of Health Reform* (New Haven, Conn., 1982); Stuart Galishoff, *Newark: The Nation's Unhealthiest City* (New Brunswick, N.J., 1988); John Duffy, *A History of Public Health in New York City, 1866–1966* (New York, 1974).

⁸ John Harley Warner, *The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885* (Cambridge, Mass., 1986).

physicians' reluctance to give up depletive therapies stemmed from their greater professional insecurity.

One of the concerns that emerges when we emphasize placing medicine within particular contexts of time and place is that of representativeness. When historians focused on theories, they assumed that ideas could exist universally, crossing state or national boundaries. But the essence of social history is the view that experience is embedded in specific historical settings. How far can one generalize from one's own data? Do Cincinnati and Boston describe the experience elsewhere in the United States? Warner's identification of regional variations in therapeutics, which he has also uncovered in a study of southern medical distinctiveness,⁹ reminds us to beware of attempts to universalize historical conclusions. The first category of work examined here, epitomized by the work of Loudon, Marland, and Warner, broadens the documentation of variation among practitioners and shows how healing practices reflected conditions in the communities in which they occurred.

ANOTHER HISTORIAN WHOSE WORK, LIKE WARNER'S, interweaves scientific and social concerns, is Charles Rosenberg.¹⁰ In *The Care of Strangers: The Rise of America's Hospital System*, a wide-ranging, comprehensive, and original study, Rosenberg traces changes in American hospitals from their function as refuges for the urban poor, institutions of charity and paternalism, to their modern form as centers of science and expertise.¹¹ This is obviously a complex story, and Rosenberg develops it by depicting the hospital as a microcosm of the world outside its walls. Although Rosenberg is primarily concerned with the role of physicians and the "culture of medicine" they created within the hospital, and fits into my first thematic category, he also understands that life on the wards owed much to nurses and other hospital workers and to patients, and in this respect he represents the second, more patient-oriented, category of study. Indeed, health care practices themselves bridged the two worlds, one inhabited by medical practitioners and the second by the sick people they served.

Rosenberg traces hospital development through the nineteenth century and into the twentieth to illustrate shifts over time that paralleled changes in cultural values as well as medical knowledge. He often uses sources derived from non-physicians, thus pointing to the depth a social history approach adds to analysis of medical institutions and practices. In his discussion of early nineteenth-century hospital life, Rosenberg draws effectively on the diary of a Presbyterian minister, Ezra Stiles Ely, who preached to the sick in New York City's almshouse hospital. The social distance between Ely and the inmates was immediately evident: he found the hospital a "grand receptacle of blasted, withered, dying, females" [prostitutes] who seemed to mock his teaching from their "beds of disease, planted with thorns." On at least one

⁹ John Harley Warner, "The Idea of Southern Medical Distinctiveness: Medical Knowledge and Practice in the Old South," in *Science and Medicine in the Old South*, Ronald L. Numbers and Todd L. Savitt, eds. (Baton Rouge, La., 1989). For other southern studies, see John Ettling, *The Germ of Laziness* (Cambridge, Mass., 1981); and Todd L. Savitt, *Medicine and Slavery: The Diseases and Health Care of Blacks in Antebellum Virginia* (Urbana, Ill., 1978).

¹⁰ Charles E. Rosenberg, *The Care of Strangers: The Rise of America's Hospital System* (New York, 1987).

¹¹ Earlier hospital studies on which Rosenberg builds are Morris J. Vogel, *The Invention of the Modern Hospital: Boston 1870-1930* (Chicago, 1980); and David Rosner, *A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885-1915* (Cambridge, 1982). For particularly creative use of institutional records, see Martin S. Pernick, *A Calculus of Suffering: Pain, Professionalism, and Anesthesia in Nineteenth-Century America* (New York, 1985).

occasion, Rosenberg tells us, Ely "drew back as a woman reached out to touch him in an unexpected gesture of warmth."¹² At the other end of the century, Rosenberg uses the letters of a student nurse, Katherine Hammond, at Johns Hopkins Hospital, to illustrate that, although trained nurses had considerably more sympathy than Ely for their patients, some social distinctions between patients and staff also existed. A young Italian patient evoked this from the nurse: "He is filthy like all Italians—and sly too. He steals the peaches out of the other men's table . . . eats them and hides the stones under his mattress."¹³ Rosenberg concludes from such sources that cultural hierarchies from the outside world intruded into the hospital, mirroring larger social realities as they marked the quality of life and influenced medical care within hospitals. He portrays an integrative story of the many factors that were part of the application of medical science within healing institutions.

Ellen Dwyer's *Homes for the Mad: Life inside Two Nineteenth-Century Asylums* is a more narrowly focused, hospital-based study that also draws heavily on the perspectives of all members of the institution "family."¹⁴ Dwyer compares two New York institutions for the insane, Utica, built in 1843 to serve acute, short-term mental illnesses, and Willard, opened in 1869 with the purpose of providing long-term care for the chronically ill. Dwyer depicts the influence of the world outside the walls by demonstrating how patients' families and social organizations shaped definitions of mental illness. More important, she portrays the world within the walls by analyzing each constituency in turn—medical superintendents, attending physicians, lay attendants, and patients and their families—and comparing the various perspectives. Although she was not the first to focus on such sources, Dwyer's extensive use of patient case histories, not previously sufficiently exploited by historians of mental health, provides a compelling account of asylum social hierarchies and their effect on treatment procedures.

UNTIL THE TWENTIETH CENTURY, most interactions between healer and patient took place in the home. In order to examine fully the role of the patient, or the lay public, in defining health practices, therefore, historians have devised strategies to make audible the noninstitutionalized patient voice. In *Sickness and in Health: The British Experience*, Roy Porter and Dorothy Porter's account of the "long eighteenth century" (the period 1650–1850), explores the "cultures" of health, sickness, and suffering as depicted in the perceptions, attitudes, and responses of sick people.¹⁵ Long on good stories, the book is full of poignant and evocative accounts by people who lived during times of high morbidity and mortality and understood their risk. The Porters quote extensively from diaries and letters of sick people, most often from prominent people whose accounts have previously been published, which

¹² Rosenberg, *Care of Strangers*, 15, 16.

¹³ Rosenberg, *Care of Strangers*, 309.

¹⁴ Ellen Dwyer, *Homes for the Mad: Life inside Two Nineteenth-Century Asylums* (New Brunswick, N.J., 1987). Along similar lines, see Nancy Tomes, *A Generous Confidence: Thomas Story Kirkbride and the Art of Asylum Keeping, 1840–1883* (Cambridge, 1984); Gerald Grob, *Mental Institutions in America: Social Policy to 1875* (New York, 1973); and his *Mental Illness and American Society, 1875–1940* (New Haven, Conn., 1983). See also Michael MacDonald, *Mystical Bedlam: Madness, Anxiety, and Healing in Seventeenth-Century England* (Cambridge, 1981); Anne Digby, *Madness, Morality and Medicine: A Study of the York Retreat, 1796–1914* (Cambridge, 1985); and Roy Porter, *Mind-For'd Manacles: A History of Madness in England from the Restoration to the Regency* (Cambridge, Mass., 1987).

¹⁵ Roy Porter and Dorothy Porter, *In Sickness and in Health: The British Experience* (New York, 1989); see also their *Patient's Progress: The Dialectics of Doctoring in Eighteenth-Century England* (Cambridge, 1989).

reveal a variety of health problems, from Fanny Burney's account of her mastectomy in 1810 (without the benefit of anesthesia) to James Woodforde's less serious complaints of 1776:

My tooth pained me all night, got up a little after 5 this morning, & sent for one Reeves a man who draws teeth in this parish and about 7 he came and drew my tooth, but shockingly bad indeed, he broke away a great piece of my gum and broke one of the fangs of the tooth, it gave me exquisite pain all the day after, and my Face was swelled prodigiously in the evening and much pain. Very bad and in much pain the whole day long. Gave the old man that drew it however 0.2.6. He is too old, I think, to draw teeth, can't see very well.¹⁶

Pain and illness were ubiquitous. When sick, most people fought back with home remedies, regimens of diet, sleep, exercise, travel; or they sought out therapeutic interventions from a wide variety of practitioners. What they experienced can only be learned from their own point of view.

Interjecting the voices of those on whom healers plied their trade alters some of our basic assumptions of medical history, most especially those taking for granted the dominance of the physician in healing interactions.¹⁷ It becomes clear from the Porters' work that medicine most often was characterized by a two-way exchange between the sick and their healers. Diagnoses and treatments were negotiated in a dialogue, which was not solely the province of medical practitioners. The interactive nature of medicine demonstrated in this book points up the continued vitality of Sigerist's fifty-year-old dictum to include both sides of the patient-doctor interchange for a full understanding of healing practices.

ANOTHER QUESTION THAT SCHOLARS ARE ADDRESSING, and are likely to continue to ponder for decades ahead, concerns the mosaic of sickness and healing experiences created by national, religious, racial, ethnic, class, or gender differences.¹⁸ The studies that address race, class, and gender recognize that not all people, even those living in close proximity, share the same experiences; therefore the need to interpret health care experiences differently for different population subgroups. These studies also delineate the influence of cultural values on professional development and the delivery of medical care. David McBride's book, *Integrating the City of Medicine: Blacks in Philadelphia Health Care, 1910–1965*, examines the impact of racial divisions on health care.¹⁹ McBride views the history of black health care in Philadelphia as a move from isolation to eventual integration within the mainstream. McBride divides the fifty-five years under his scrutiny into three parts: the period before World War II, which he describes as having two worlds of medicine in the city (white and black); the postwar period through the early 1950s, which he identifies as a time when racial barriers were challenged; and the 1960s, a time when blacks, with the help of the civil rights movement, achieved access to

¹⁶ Porter and Porter, *In Sickness and in Health*, 118–19.

¹⁷ See Judith Walzer Leavitt, *Brought to Bed: Childbearing in America, 1750–1950* (New York, 1986), for an example of including birthing women's perspective in the dialogue over childbirth practices.

¹⁸ National and religious differences of experience need examination, and work on both is now being done. See Ronald L. Numbers and Darrell W. Amundsen, eds., *Caring and Curing: Health and Medicine in the Western Religious Traditions* (New York, 1986). See also the papers presented at the meeting of the Organization of American Historians, 1988, by Nancy Tomes and John Warner.

¹⁹ David McBride, *Integrating the City of Medicine: Blacks in Philadelphia Health Care, 1910–1965* (Philadelphia, 1989). The discussion of race only in terms of African-Americans in this review is not meant to discount the importance of Hispanics and other racial and ethnic groups to medical history. It does, however, point to the group that has been the focus of recent work.

previously segregated institutions. McBride emphasizes how the healing and sickness experiences of black people in this northern city were shaped in part by whites' self-interest; that is, only when whites perceived a threat to themselves (through black domestic workers bringing disease into their homes, for example) did they attempt to ameliorate high mortality and morbidity among the black population. He also recognizes the important roles played by black activism itself. Unfortunately for our understanding of how racism interacted with class interests, McBride does not delve into differences within the black community (divisions over, for example, the formation of the second black hospital, or over the value of integration itself). Nonetheless, studies such as McBride's are moving scholars in the essential direction of seeing the interactions between a varied social world and a varied medical one.

A book that considers cultural attitudes toward both class and race as factors affecting health and health care is Edward Beardsley, *A History of Neglect: Health Care for Blacks and Mill Workers in the Twentieth-Century South*.²⁰ Beardsley combines a study of black southerners with one of white cotton-mill workers because he views both groups as victims of a social system that stigmatized and ignored them, leading to appalling health problems and making ameliorative efforts particularly difficult. In Beardsley's analysis, poverty was a cause and effect of ill health for both groups. Racism and segregation added to the health problems suffered by the black southern population, while a strict system of paternalism and social isolation in the mill villages and factories similarly increased the health problems of that segment of the white population. Government agencies neglected black health care; textile mill owners neglected their workers' health care.

Although seeing the problems of both groups as parallel in many ways, Beardsley discusses them separately in the chapters of the book to make clear their differences. Racism determined that the two groups lived and worked separately from one another for most of the period studied, and this segregation also led to distinctions in health care provisions. Mill hands, despite the effects of profit seeking and paternalism that characterized their employers' actions, received considerably more private professional care than blacks, poor or not. The one unfortunate aspect of the decision to separate the strands of analysis in the book is that Beardsley cannot examine how class and race simultaneously might be factors in understanding the health-related experiences within the black community. Even though he uses considerable space uncovering the effects of racism on the careers of black doctors and their attempts to alleviate the desperate health situation of poor blacks, Beardsley does not analyze middle-class blacks' own health situation. Nonetheless, he examines the changing health status of blacks and white mill workers within a comprehensive social context of southern life, viewing medical and social history as working together.

The approach that analyzes how gender, class, and race together affected health care, and one that provides a view of how historians will be pursuing the subject into the 1990s, can be seen in both Susan M. Reverby's *Ordered to Care: The Dilemma of American Nursing, 1850–1945* and Darlene Clark Hine's *Black Women in White: Racial Conflict and Cooperation in the Nursing Profession, 1890–1950*.²¹ The historically

²⁰ Edward H. Beardsley, *A History of Neglect: Health Care for Blacks and Mill Workers in the Twentieth-Century South* (Knoxville, Tenn., 1987).

²¹ Susan M. Reverby, *Ordered to Care: The Dilemma of American Nursing, 1850–1945* (Cambridge, 1987); and Darlene Clark Hine, *Black Women in White: Racial Conflict and Cooperation in the Nursing Profession*,

gendered character of nursing as an occupation immediately raised the question for both of these historians of whether and to what extent the profession was shaped by the sex of its practitioners. More precisely, did—or, how much did—the cultural values and status of women determine the subservient position of nurses and their specific functions in the division of labor in relation to the predominantly male medical profession? Reverby and Hine each identify an important component of nurses' position defined by gender. Reverby's analysis of the central dilemma of nursing is particularly acute; she views it as a profession whose job is to care (a female-defined task) in a society that does not value caring. Both historians, however, also see that gender alone does not describe professional development. Reverby concentrates on understanding the obstacles to unity on gender issues posed by class differences among nurses—especially between nursing leaders and rank-and-file nurses. Hine emphasizes that racism among white nurses as well as in the culture at large determined separate institutions (schools and hospitals) for black nurses and uniquely shaped their experiences.

Together, these two perspectives add considerably to our understanding of nursing and, by extension, offer insights for the study of other branches of healing. For example, Reverby discusses rationales for training student nurses by having them work many hours on the wards. This work was thought necessary in order to inculcate moral behavior, the womanly duties of service, and submission to physicians' authority. At the same time, the pupils provided free labor for the hospital. Hine's perspective is based on the African-American experience. She notes the economic and social value to the black population of this unpaid service of student nurses, and she sees such work as justified by the desperate health situation of black Americans. The unpaid labor was also justifiable because the community held these women in high regard once they graduated, a regard not mirrored in the white community. In addition, Hine's study reveals that inclusion of racial differences within nursing history alters professional periodization: although hospitals hired white graduate nurses as staff nurses during the Great Depression, it was not until the 1960s, under pressure from the civil rights movement, that black graduate nurses had access to such jobs. Thus it is not just in relationship to male privilege that we must view developments within nursing. Only within the fuller social context provided by including class and race as well as gender can we begin to uncover the complexity and variation of past experience.

These examples, and both books are rich with others, make obvious the need for attention to differing social groups if we are to improve historical knowledge, and they also point to the necessity of critical analysis of what we find. We need to do more than identify differences; we need to comprehend how social values and prejudice have affected experience and use that insight in our analyses of the processes of change. As the example from nursing history illustrated, alongside gendered constructions of women's work and class divisions, racism permeated health and healing experiences of whites and blacks and influenced professional development in very basic ways. It may be too much to ask any individual historian to integrate adequately all approaches and all peoples within a single study, but it is essential that we learn to look broadly at our own work and the work of others and begin the long-term task of fitting together the complex pieces of the puzzle of health care history.

1890–1950 (Bloomington, Ind., 1989). See also, on the history of nursing, Barbara Melosh, *"The Physician's Hand": Work Culture and Conflict in American Nursing* (Philadelphia, 1982).

MOST HISTORIANS MAKE THE TASK MANAGEABLE by limiting their studies in various ways. Gloria Moldow considers race and sex as factors influencing professional development in a specific location in *Women Doctors in Gilded-Age Washington: Race, Gender, and Professionalization*.²² Focusing on Washington, D.C., between 1870 and 1900, Moldow examines and compares the social origin, education, and practice of almost two hundred women physicians, black and white. Although Howard University provided Washington with a unique opportunity for educational experiences across racial and sexual boundaries, Moldow finds that both race and gender prejudice, working concurrently in the case of black women, curtailed career development of women physicians in the District. Moldow's conclusion that Washington provided a particularly harsh climate for female physicians of both races demands close comparison with experiences in other cities. Certainly, the District's lack of an all-female medical school (which many other large urban centers had in this period) must have made the women's path more difficult. Moldow's clear demonstration of racial barriers seen in the Washington context is particularly telling. Despite the presence of Howard University and Washington's large number of black health professionals, black doctors in the city faced the same exclusion as their counterparts elsewhere. White male medical societies, for example, excluded all blacks as well as white women from their membership. When the societies ultimately came to accept white women, they retained the racial barriers. White women, too, excluded black women from their associations, while black men excluded black women on the basis of gender, leaving black women triply outcast and bereft of opportunities for professional development. The illumination of race and gender issues in this local study provides a model for future investigations.

Another strategy to limit the focus while broadening the analytical approach is superbly demonstrated in Joan Jacobs Brumberg's study of a single disease, *Fasting Girls: The Emergence of Anorexia Nervosa as a Modern Disease*.²³ Brumberg addresses the relationships between culture and biology through an examination of anorexia nervosa, an illness diagnosed in the modern period almost exclusively in adolescent females. Within Sigerist's paradigm of weighing the importance of both ideas and practice, Brumberg incorporates the social as well as the biomedical explanations to analyze the history of this disease. Identified in its modern form in Britain and France during the 1870s, anorexia nervosa depended on a new medical comprehension of the place of food at the center of communication and love in the middle-class family. Brumberg argues that, since adolescent girls particularly felt the simultaneous family pressures of dependence and expectation, they responded in a language that made sense in the context, by refusing food (love) proffered by their parents. Brumberg considers the symbolic meanings of food-refusing behavior, seeing it always as a product of the interaction between social pressures and values, psychological responses to particular situations, and biomedical factors of the disease itself. Culture, in her analysis, and the pressures on adolescent girls, predominate historically in the "recruitment" to the disease; physical and biological forces take control during the "career" of the disease. The insight we gain through

²² Gloria Moldow, *Women Doctors in Gilded-Age Washington: Race, Gender, and Professionalization* (Urbana, Ill., 1987). For more on the history of women in medicine, see Regina Morantz-Sanchez, *Sympathy and Science: Women Physicians in American Medicine* (New York, 1985). An excellent biography of a woman physician and her social context is Barbara Sicherman, *Alice Hamilton: A Life in Letters* (Cambridge, Mass., 1984).

²³ Joan Jacobs Brumberg, *Fasting Girls: The Emergence of Anorexia Nervosa as a Modern Disease* (Cambridge, Mass., 1988).

Brumberg's sensitive gender analysis of the effects of culture on young women, the influence of their family situations on their bodies, and their physicians' views and treatment of them provides a standard we can apply in the study of other diseases.

Not only have historians extended their base of study from elite physicians and their texts to include all healers and all patients and to emphasize the practice of healing as well as its theoretical underpinnings, but they recently have made inroads into understanding the differences among various populations. Variations of experience connote more than just the pluralist culture in which healing interactions occur; they also reflect inequalities and power differentials. Thus at the same time that historians explore the extent to which patients wielded decision-making authority and control over the therapies they received from their medical practitioners, they see how the specialized knowledge of the healers may have given them an advantage over their patients. Similarly, historians understand that race and gender worked not just to differentiate people from one another but also implied a power difference that greatly affected experience. Hierarchies of medical knowledge and social order become essential ingredients of analysis in the history of medicine in its late twentieth-century incarnation.

INTELLECTUAL CURRENTS IN THE ACADEMY are pushing scholars today toward a closer examination of influential written texts, a seeming throwback to earlier emphases privileging the significance of the ideas of a medical elite who wrote the textbooks and published in the leading journals. Certainly, if this tendency were again to predominate in the history of medicine, the voices of patients, of women, blacks, and non-elite healers would again disappear, since these groups do not produce hegemonic literature. Such a reversion would be regrettable; although I believe deconstructing primary sources of all kinds constitutes the backbone of good historical technique. If we can accept the productions of non-elite groups as relevant to medical histories—for example, family diaries and letters, garden contents, household recipes, papers of healers of all types—and place all available documents within a context of power relations and culture, we will be doing what good historians should do, reading deeply for the fullest possible understandings. We must expand our notions of which texts are valuable, just as we have become more inclusive in our notions of which groups in the healing process are worthy of attention.

Laurel Thatcher Ulrich's book, *A Midwife's Tale: The Life of Martha Ballard, Based on Her Diary, 1785–1812*,²⁴ does an exemplary job of reading newly valued texts within a broad social and historical context. The text she chose, a midwife's diary, represents a group of people whose voices have only recently been taken seriously in the history of medicine. Ulrich's method is a powerful indication of what we can learn about healing and its theory and practice from a single text when it is both closely analyzed and put alongside a wide variety of other historical documents and within its particular community setting. Through the eyes of a single practitioner, Martha Ballard, a midwife—not a physician—from Hallowell, Maine, Ulrich helps us to see anew the place of healing within the social fabric of a community.

Through the pages of the diary, from which Ulrich quotes sufficiently to give

²⁴ Laurel Thatcher Ulrich, *A Midwife's Tale: The Life of Martha Ballard, Based on Her Diary, 1785–1812* (New York, 1990).

readers a sense of its "exhaustive, repetitious dailiness,"²⁵ unfolding before us are the comings and goings of the private and public figures, and the ways in which healing crossed back and forth between the two worlds. Martha Ballard gave birth to 9 babies; she helped other women deliver 998 (814 recorded during the years of her diary), in the process fording treacherous rivers, making her way through blinding snowstorms, and leaving her family for days at a time. But Ballard did much more than aid women in their childbirths; she carried out a variety of other healing activities, including dressing wounds, treating fevers, setting bones, and applying medicines of various kinds. (Ulrich provides a glossary of the medical ingredients that Ballard used in her practice; most of them she grew in her own garden or gathered locally.) Through all of these encounters, Ballard showed not only her expertise but also her connection to the people she served. She sat beside them in church, she shared gardening, baking, and other domestic chores with them, and she helped them grieve over and bury their dead as well as celebrate family and community events. Ballard also regularly interacted with the physicians and other healers in the town. She referred patients and problems to them or they to her, a process that indicated mutual respect and cooperation in their relationships. By viewing Ballard and her healing activities within the social web of the town and its life, Ulrich establishes that medical practice was an intimate part of community life, public and private.

Because Ballard was a woman, and because, in her culture, activities were often defined as either women's or men's, her diary greatly increases our perspective on the eighteenth and early nineteenth-century world. One of the freshest insights Ulrich has in store for readers of this book, and there are many, is that provided by an explicit comparison of Ballard with male writers from the same town, one of whom was a physician. Dr. Daniel Cony wrote about obstetrics in Hallowell in a year during which Martha Ballard delivered 60 percent of the babies, and he never once mentioned midwives. Ballard's diary entries, on the other hand, not only mention Cony and put male medical attendance at childbirth in its place as supplementary to female midwives' but also give a picture of general healing practices in which doctors and community women interacted and shared knowledge. Similarly, the account book of the wealthiest man in town provides excellent material on the economy of the region but excludes women from any economic role whatsoever. Again, Ballard's diary provides a corrective and reveals information about textile production as well as healing, the missing work of women. A third male Hallowell writer, diarist Henry Sewall, only occasionally mentioned local domestic happenings, concentrating instead on national political events. Ballard delivered Sewall's wife eight times: his diary records her presence at only three births, never mentioning the fees he paid her, the names of the other women present, or the complications that attended the deliveries (all of which we learn from Ballard). Sewall's diary has little to say about any of the women of the community or their activities; he neglected even to include his wife's bonnet-making. These comparisons illustrate that we cannot understand healing practices, which crossed the boundaries between the domestic and the public, without information from both worlds. Leaving out women's perspectives heretofore has distorted the history of medical practice.

Ulrich's book reflects the trends in recent historiography identified in this review essay. It includes and values all groups of practitioners, it incorporates the roles of

²⁵ Ulrich, *Midwife's Tale*, 9.

patients and community, and it analyzes different kinds of healing experienced, especially in terms of gender. Ulrich integrates traditional perspectives, gained from using, for example, physicians' published or unpublished writing, with newer ones, culled from such sources as Ballard's diary and the town records, all the while comparing and contrasting points of view. In the process, she transforms the previous picture of late eighteenth-century healing practices. The book points the way for historians of the coming decades, in its intricate weaving of textual narrative and contextual analysis.

HISTORIANS WRITING ABOUT HEALTH AND HEALING, as exemplified in this essay, are adding the complexity and detail that Sigerist had hoped would emerge when historians broadened their vision. No longer do we view physicians only or primarily as medical innovators and discoverers. We see healers, like Martha Ballard, as integral to their communities, functioning as private and public persons, and bound by the values and experiences of their time and place. We see the relationship between healers and the people who sought their help as crucial to understanding medical practice. We see the sick people themselves, wielding various degrees of control, as partners in the healing dialogue. We see how social inequalities have affected healing practices. And we see that an analysis with different points of view can enrich our historical perspective. We see, in other words, health and healing as subjects for wide-ranging historical analysis, in the able hands of a variety of scholars, all of whom are succeeding in expanding our knowledge of the past.

Reviews of Books

GENERAL

JONATHAN CULLER. *Framing the Sign: Criticism and Its Institutions*. (Oklahoma Project for Discourse and Theory.) Norman: University of Oklahoma Press. 1988. Pp. xviii, 237. \$29.95.

For the past fifteen years literary critics and theorists in the United States have grappled with the fundamental assumptions of their discipline. The discussions have become so heated and controversial that many literary theorists no longer consider themselves a part of a definable academic "discipline" and instead designate their professional activities as "projects." The most radical of the literary theorists seriously question whether or not it is epistemologically possible to determine what an author intended and, even more difficult, to project what meanings a reader derives from a particular text. Led by the "deconstruction" of Paul de Man and Jacques Derrida, the most skeptical theorists despair that meaningful literary discourse is any longer possible, given the ambiguity and confusion within language itself and the distortions of language caused by inherent, perhaps unavoidable, class, gender, and racial biases. Although historians have been aware of the "postmodern" ferment among literary theorists, until recently, we have remained happy, if largely ignorant, on the sidelines. With the publication of Peter Novick's *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (1988), however, and the theoretical discussions published in recent issues of the *AHR* (June and October 1989), few should doubt that our moment of tranquil isolation has ended.

Jonathan Culler has anticipated our interest if not answered all of our questions. A series of essays written over the past fifteen years, his book includes a history of American literary theory since World War I; an examination of the philosophical and religious implications of semiotic-based literary theory; sympathetic but critical discussions of key contributors to the current debate (Gaston Bachelard, William Empson, Paul de Man, Jacques Derrida, Stanley Fish, and Jürgen Habermas); and case studies of law, rubbish, and tourism that suggest the interpretive possibilities offered by various semiotic approaches, including neo-historicism. Culler's intent, it seems, is to demystify and defuse the esoteric and often polemical discussions of

semiotic theory for the benefit of the uninitiated, to introduce us to its cast of important characters, and to sketch out its history—all in the hope of dispelling its reputation as yet another arcane intellectual craze, signifying nothing.

Culler has succeeded and much more, incidentally demonstrating that meaningful intellectual discourse is possible. Apart from his notable theoretical abilities, Culler is a skilled historian. Indeed, one of his central interests is the interpretive importance of the cultural and institutional "framing" of language and literature or what conventionally passes for cultural history. Because he develops with detailed subtlety a discrete argument or subject in each of the eleven essays, even a brief summary of the individual essays is inadequate. Suffice it to say that each of the eleven essays is important and stylistically accessible. Together they offer historians of all persuasions a number of provocative and constructive challenges that do not ask us to take leave of our senses or to abandon logic.

The lead essay, "Literary Criticism and the American University," is of special interest to historians. In thirty-seven pages Culler sketches the development of literary criticism and theory in the United States since World War I, identifies the New Critics as the authors of the profession's dated but still powerful synthesis, and succinctly outlines the current postmodern debate. For the uninitiated but interested, it is an extremely useful introduction to what from the outside seems a daunting semantic and epistemological thicket.

Having diagnosed the "problem" of the English profession in a manner that resembles the "problem" now confronting historians, Culler insists that the current epistemological cacophony enriches rather than confuses current scholarship. Deterred neither by epistemological skeptics nor Neo-Conservative defenders of the Western tradition, he declares that the new theoretical emphasis within criticism is consistent with the bureaucratic character of the university and its research mission that demands specialization and published results. Moreover, semiotic-based theories, when joined with neo-historicists' recognition of the significance of historical and institutional framing and the necessary deconstruction of the literary canon, open the way for the reconstruction of literary theory and criticism along interdisciplinary, pluralistic, nonelitist,

and multicultural avenues. Although Culler does not offer any new synthesis to replace the battered "Humpty Dumpties" of New Criticism and Neo-Conservatism, his conclusion is upbeat and undaunted.

An important and provocative work, this book is written in a clear, readable style, making no claim to be the last or definitive work on the subject. Read in conjunction with Novick's *That Noble Dream* and the *AHR* forums and review articles, Culler's study offers historians tantalizing possibilities indeed.

WILLIAM B. SCOTT
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ALBERT COOK. *History/Writing*. New York: Cambridge University Press. 1988. Pp. x, 275. \$39.50.

Albert Cook is an eminent and widely published scholar in comparative literature, fluent in half a dozen languages, enormously learned, and fully cognizant of the most arcane and sophisticated semiotic and hermeneutic theories. In this work Cook ventures into the historian's territory, examining the writings of a wide variety of historians ranging from Herodotus to Fernand Braudel. His purpose is to uncover the literary dimensions of historical texts and to disclose the rhetorical devices that historians sometimes unconsciously employ. Cook also devotes considerable attention to texts that are not normally classified by practicing historians as "historical," such as the Torah and the New Testament.

Drawing primarily on the methods of deconstructionists, Cook offers subtle and often perceptive comments on the nature of fact, on the particular versus the general, on the synchronic versus the diachronic in historical writing, and on how the historian uses detail. Cook also draws a distinction between the argued case and the chronicle with sensitivity and argues convincingly that all writing of history must be synecdochic.

One might question Cook's principles of selection and his arrangement of the authors—some identifiably historians, others not—whom he chooses to discuss. To apply terminology that he uses in his own work to describe historical method, Cook's "strategies of inclusion" (p. 137) are highly eccentric. The reasons behind his digressions are often difficult to decipher. For example, in his first substantive chapter Cook analyzes in some detail and with a good deal of sympathy Ezra Pound's oracular interpretation of several thousand years of Chinese history in the *Cantos*, hardly what most of us understand as history. In the next three chapters Cook leaps from a sensitive and thoughtful discussion of Thucydides to a commentary on Edward Gibbon. He then breaks chronology by devoting a chapter to Tacitus.

Only four pages, sandwiched between commentaries on Max Weber and Giambattista Vico, are dedicated to Marxian historiography. Yet Oswald Spengler, generally regarded as a third-rate thinker whose only claim

to fame is the way *The Decline of the West* reflects the global crisis generated by World War I, and who would be lucky to receive a paragraph in many historiographical studies, is one of Cook's favorite "historians" and receives eight pages of spirited defense. In Cook's view Spengler is much superior to Arnold Toynbee and, at his best, is matched only by historians of the caliber of Thucydides, Tacitus, or Gibbon.

The territorial instincts of most historians are not particularly strong, and few would question borrowings from or lendings to other disciplines. Historians are used to clear and ordered argument, however, and many attempt in their writing to communicate with a broad audience of nonspecialists. For this reason some might wish that Cook had stayed at home or pursued his investigations in another discipline. Even intellectual historians comfortable with the writings of a Haydn White, a Michel Foucault, or a Roland Barthes might take umbrage at such sentences as "in fact the emplotment can itself be taken as an overall illustration and also as an implicitly dialectical sublation, of a large event, multiply aspectualized so that it may appear plausibly as a single large event with all its contributory enchainments" (p. 9).

Although this work lacks the sustained elegance of Marc Bloch's *The Historian's Craft* (1949) or the critical bite of Dominick LaCapra's *History and Criticism* (1985), it is sprinkled with brilliant and provocative formulations, and the patient reader willing to forgive such sentences as the one cited above can uncover many intriguing insights. Professional historians will perhaps be led to a new self-consciousness and a heightened sensitivity to the rhetorical devices that even the most quantitative and professedly scientific among us commonly implant into our writing. Understanding more fully the tropes we all employ could be an unnerving experience.

Whether the greater awareness of the literary dimensions of the historical enterprise that Cook provides will actually affect the practice of historians in the future is a difficult question to answer. My own guess is that Cook will not guide us to better historical writing, but he may perhaps incite us to more careful thinking about what we are writing and how and why we write.

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RAYMOND MARTIN. *The Past within Us: An Empirical Approach to Philosophy of History*. Princeton: Princeton University Press. 1989. Pp. xiii, 163. \$25.00.

Raymond Martin's book is an impressive critique of the analytic and positivistic philosophy of history and a persuasive argument for a new empirical approach. Martin believes that historians' typical lack of interest in analytic philosophy of history is understandable. Instead of basing their analysis on what historians actually do or claim to do, philosophers have attempted to develop a priori and conceptual accounts of what it is

that historians should be doing in principle. Martin, however, proposes to begin with an analysis of actual historical work and from that build an empirical philosophy of history.

Martin starts with a brief history of speculative, critical, and analytic philosophies of history. Analytic philosophy of history has emphasized the problems of objectivity and explanation. Martin argues that both the positivists and their humanistic critics have based their arguments on the false assumption that historians ought to defend covering laws or generalizations resembling them for each of their specific explanations. If historians could not justify their explanations, then the explanations could not be defended rationally. These assumptions, however, do not represent the ways historians usually argue. Historians tend to argue that their explanation is better than competing ones, not that it is sufficient. Martin believes that philosophers need to examine case studies of how historians actually justify their explanations if the philosophy of history is to move beyond the increasingly sterile positivistic arguments.

In demonstrating his empirical philosophy of history, Martin examines "explanatory competition," "causal weighting," and "conceptual and empirical subjectivism." He argues that historians usually defend their explanations by arguing that they are better than competing versions. They may offer positive reasons to support their account or criticize and undermine competing accounts. Martin demonstrates this by analyzing explanations for the collapse of the classic Lowland Maya civilization. His analysis of causal weighting—how historians assign causes to particular events—is based on a study of the causes proposed for the collapse of the western Roman Empire. He concedes that the problem of causation is complex, and he offers only a few suggestions toward clarifying the problem. Finally, he asks whether history is necessarily conceptually or empirically subjective and concludes that there has never been a successful argument for the necessary subjectivity of historical studies. The arguments that have been made are either trivial or not persuasive.

Martin also defends a "modest empirical subjectivism" by examining the relationship of the Gospels of Matthew, Mark, and Luke. This synoptic problem requires historians to exercise judgment because we are unlikely ever to have enough information to resolve it definitively. Martin concludes that, in the absence of better alternatives, we are justified in relying on historians' judgments. History will probably never be able to escape this kind of subjectivity, and this may mark the difference between history and science. Martin ends with an appendix on historical counterexamples in which he concludes that a positivist analysis of this phenomenon is probably correct, but it has been largely ignored in philosophical literature.

Martin has packed a great deal into a short book. His analysis of why historians disregard analytic philosophy of history is convincing. His examples of an empirical

approach to the philosophy of history should also persuade us that we have something to learn from philosophers. He demonstrates through his case studies that useful generalizations can be drawn about what historians do. In addition, he shows how empirical philosophical analysis can help clarify historical argumentation. Written from a philosophical perspective, Martin's book is accessible to any historian interested in a better understanding of the discipline. His prose is clear, his arguments are firmly rooted in the historical examples, his use of philosophical notation is limited, and his conclusions are persuasive. This book is an excellent introduction to an empirical philosophy of history.

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HENRI-JEAN MARTIN. *Histoire et pouvoirs de l'écrit*. Foreword by PIERRE CHAUNU. Assisted by BRUNO DELMAS. (Histoire et décadence). Paris: Librairie Académique Perrin. 1988. Pp. 518. 200 fr.

Thirty years after coauthoring the justly celebrated *L'Apparition du livre* (which he wrote almost entirely by himself), Henri-Jean Martin has brought out a much more wide-ranging synthesis covering the fate of writing from the first cave drawings to the proliferation of electronic media. Lucien Febvre and Martin's work on the advent of printing had been commissioned by Henri Berr for his *Evolution de l'humanité* series, both to serve as a bridge between sections devoted to medieval and modern times and to delineate a "decisive step" in an evolutionary process, which was initiated by *Le Langage* (vol. 3) and followed by *Le Journal* (vol. 94).

Pierre Chaunu, editor of the new series in which this book appears, provides a preface that opposes evolutionary schemes. He stresses the trade-offs entailed in each of Berr's "decisive steps" and describes the spread of the printed word as a "quasi-cancerous growth within the realm of discourse." He also asserts that the work being introduced is "one of the greatest works of history ever written," thereby raising expectations that are just about impossible to fulfill (p. 17).

In my opinion, the book in question, although certainly meritorious, falls short of deserving such extravagant praise. It is a remarkable achievement to depict (and to compress into a single volume) developments affecting the fate of a major human activity over the course of millennia and on a global scale. But, as might be expected, this ambitious undertaking stretches the author's reach beyond his grasp. Although Martin has enlisted collaborators to help with some exotic material and cites a large secondary literature when dealing with esoteric topics himself, coverage is relatively thin and patchy on regions outside Western Europe and on non-alphabetic writing systems. Mathematical notation receives sketchy treatment; literacy, not numeracy, is stressed. Large sections seem more relevant to a history of publishing (and to the multivolume *Histoire de l'edi-*

tion française, which Martin recently coedited) than to the topic at hand. Throughout the work, the French experience is taken as paradigmatic. (Discussion of the typewriter keyboard, for example, deals with the failure of Anglo-American arrangements to conform to the idiosyncrasies of French.) But it would be unfair to dismiss the book as just another historical survey "made in France."

The organization of chapters, although basically chronological and in accord with conventional periodization, is sufficiently flexible to allow room for discussion of significant topics, such as the shifting balance between orality and literacy in ancient, medieval, and early modern times. At various points, personal reflections provide a refreshing interlude within an information-laden text. There are the usual textbooks clichés (on the rising bourgeoisie), but there is also informed analysis of recent debates (over the development of silent reading, for example). Frances Yates's work on the art of memory has been thoroughly assimilated, although memory arts are played against writing systems in a rather mechanical and unoriginal way.

In addition to exploring the changing relationship between the spoken and the written, Martin also examines the way legal, religious, political, and economic institutions have interacted with diverse systems of writing. Institutional pressures become especially prominent once the author arrives at the invention of "artificial writing." On the age of Gutenberg, the grand master of French book and society studies demonstrates his unequalled command of the material. But readers who are already familiar with Martin's many other books will find little that is new and, disappointingly, too much that is old. (A discredited anecdote about a manuscript book dealer, which has led generations of historians to miscalculate the time taken to copy books by hand, is repeated without correction on page 187.) The author's concern with institutional arrangements has a negative side, entailing a tedious country-by-country assessment and constant repetition of the same location: "let us now turn to Italy" or to Germany or elsewhere. Most often, of course, we are asked to look at France. One also grows weary of repeated assertions that a given country's "hour was on hand." When the hour of England comes, incidentally, it brings with it an unacceptable number of misprints.

In contrast to the informed analysis of French institutions, the scanty passages on nineteenth-century Russia and America (which are coupled with China and Japan in a section on the world-wide spread of printing) offer bits of raw data: estimates of literacy rates in tsarist Russia; the dates of the first presses set up west of the Mississippi and of the establishment of notable firms in the eastern U.S. listed without comment. As the work approaches the modern era, one senses that criteria for inclusion and exclusion of material become increasingly unclear; at the same time the author's personal reflections become increasingly pessimistic. (Can these two phenomena be related?) After a penultimate chapter on electronic media, written by a collab-

orator, a conclusion warns of the dire consequences of the advent of mass media and the reduction of culture to merchandise. The work ends on a melancholy note, evoking late Roman attempts to preserve fragments of a shattered civilization against the barbarian onslaught. The theme of decadence first sounded in Chaunu's preface is thus reasserted in Martin's final remarks. In my opinion, Berr's evolutionary scheme was better suited to inspiring a great work of historical synthesis.

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JACOB NEUSNER. *Self-Fulfilling Prophecy: Exile and Return in the History of Judaism*. Boston: Beacon. 1987. Pp. 230. \$25.00.

Jacob Neusner's investigation of the theme of exile and return comprises another of his many penetrating analyses of Jewish history. Indisputably the most prolific interpreter of Judaism and, by any count, one of the most insightful scholars of religion, Neusner seeks the broader meaning of the Jewish experience beyond the detail, with which this book, like all of his studies, is amply endowed.

It is Neusner's thesis that the biblical period known as the Babylonian captivity, or, especially for Jews, the Babylonian exile, holds the key to the comprehension of the Jewish experience. Broadly bracketed between 586 B.C.E. (some might begin with 597) and 450 B.C.E., the traditional date for Ezra and Nehemiah, this period witnessed, on the one hand, the redaction by Jews and in part the de facto canonization of the sacred records of their past and, on the other, the consolidation of Judahite identity through reflection on the unmitigated despair of exile and the sustaining ideal of the ultimate return. The Babylonian exile became the foundation experience of the Jewish group; its theme, or, we may say, its myth of exile and return, shaped the self-perceptions of all subsequent Jewish experience and informed the variety of its successive religious formulations. Above all, in the perennial disappointments of the Jews' physical and psychological exiles, the Babylonian exile served as the archetypal loss and the return, however long deferred, as the compensatory restoration.

Tracing this paradigm to provide what he calls "a social theory of the history of Judaism" (p. 2), or, as he elsewhere states, the "histories of Judaisms" (p. 12), Neusner takes the reader through the diverse stages of Jewish experience: its constitutive or biblical period, which he appropriately calls "Judaism before Judaism" (p. 18); its formative era, corresponding to the Talmudic period, the period to which Neusner has contributed so spectacularly and to which he here in two chapters devotes his greatest attention; its classical period, from the end of the Babylonian Talmud to the modern age, symbolized by the American Constitution

and French revolution; and the modern and contemporary period, with its dislocations that occasion what Neusner calls "the Fall of Judaism and the Rise of Judaisms" (p. 181).

Though panoramic in scope, and limited in size, Neusner's study is comprehensive in its coverage of the major currents of Jewish history and their tributaries; abundant in data, which, far from burdening or obstructing, cogently illustrate his teaching; and, not least, penetrating in his treatment of the diverse movements within Judaism, especially Reform and Orthodoxy, and what he calls with daring originality the "extrasystemic heresies" (p. 199) of American Judaism and Zionism.

Characteristically interesting, scholarly, insightful, and provocative, Neusner's study offers a profound contribution to the philosophy of Jewish history and, at the same time, an engaging survey of the Jewish experience.

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DAVID VITAL. *Zionism: The Crucial Phase*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xvi, 392. \$65.00.

Zionist histories necessarily face the difficult task of combining and balancing the perspectives of both the complex web of internal Jewish politics, religion, and tradition and the arena of international politics and great power relations. The more comprehensive the treatment, the more likely this task will be accomplished with success. This volume, the third in David Vital's history of Zionism during its critical early decades, completes what unquestionably has been a brilliant success.

In all three volumes, the author promotes the primary thesis that the emergence of Zionism was nothing less than a revolution in the structure and the condition of world Jewry. In their rebellion against the condition of exile, Zionists, not socialists, are seen as the true Jewish revolutionaries. The first volume, *The Origins of Zionism* (1975), considers the fragile, early beginnings of a modern Zionist consciousness and movement from the early 1880s to the first Zionist congress in 1897. The second, *Zionism: The Formative Years* (1982), focuses on the emergence of the permanent character and features of the Zionist movement between the Basle congress of 1897 and the immediate aftermath of Theodor Herzl's death in 1904. *Zionism: The Crucial Phase* deals with the malaise in Zionist ranks after Herzl's death, which was followed by the dramatic improvement in Zionist fortunes brought on by World War I. It culminates with the achievement of a Jewish National Home in Palestine and, thus, of the return of the Jews as a people to the arena of world politics as part of the postwar settlement.

Vital sees Zionism as a revolutionary phenomenon

distinct from, rather than part of, a much larger revolutionary process of Jewish emancipation and assimilation, itself part of even larger revolutionary forces transforming Western society as a whole during the nineteenth century; although questionable, this view does not compromise the book's quality as a Zionist history. The volume completes what will certainly remain for many years to come the standard history of Zionism from its earliest beginnings to the establishment of the Jewish National Home.

This period of Zionist history is presented primarily as one of decline during the ten years between Herzl's death and the outbreak of World War I. Of particular importance is the continuing philosophical struggle between Herzl's "political" Zionism and the "practical" Zionism of his critics who were in the majority. Vital's story is also the history of a small, weak movement with few resources and little support from the Jewish and non-Jewish worlds. Zionist weakness, to some extent both a cause and an effect of the tension between political and practical Zionism, was itself a reflection of deeper tensions in the Jewish tradition regarding relations with the non-Jewish world.

Vital clearly demonstrates that, in spite of the pre-eminence of the practical Zionists during this period, Herzlian political Zionism, far from disappearing, remained an inescapable necessity before 1914 and a dominant reality in the wartime diplomacy between Chaim Weizmann's rebellious British Zionists and the British government. If Zionism barely survived the difficult years before 1914 because of practical Zionist activity in Palestine, Vital sees the political activity spearheaded by Weizmann as ultimately saving the Zionist idea and movement by achieving the National Home, the necessary first step toward the Jewish statehood and independence desired by practical and political Zionists alike. The author leaves little doubt, with his consistent criticism of the mainly central European Zionist leadership before 1914, that he credits the political strategy of the few before 1914, and of Weizmann and the British Zionists during the war, with the ultimate survival of Zionism.

The author admits that the realities of war made practical Zionism difficult and brought political Zionism of necessity to the forefront. He is thorough and insightful in his treatment of great power interests and rivalries in the Middle East during the war and the difficult and delicate relationship of a fractured Zionist movement to the intrigues of great power diplomacy. The second half of the book focuses on the emergence of Weizmann and the intricate wartime diplomacy involving his small group of British Zionists, the continental Zionist leadership of the Smaller Actions Committee in Berlin and in neutral Denmark, the anti-Zionist British Jews of the Conjoint Foreign Committee, and the British and other Allied governments. Vital correctly isolates the two key players who determined Zionist fortunes, namely, the British Zionists, motivated by the practical considerations of British power in the Middle East and the Anglophilia of

Weizmann, and the calculated self-interest of the British government, which saw Zionism as useful and worthy. Vital's assessment of Zionist and British motives in their wartime partnership, although not new, is appropriate.

If something is missing from this otherwise superb study, it is an adequate treatment of the Arab factor in the wartime Anglo-Zionist diplomacy. Although this is a Zionist history, no such history of the First World War and postwar period is complete without adequate reference to the Arabs in British and Zionist calculations. The suggestion that the Arab delegations in Paris possessed advantages over the Zionists, given the Balfour Declaration, Allied postwar ambitions in the Middle East, and the European character of Zionism and its leaders, is difficult to accept. There is precious little on Arab demands at Paris, and on Anglo-Zionist reaction to those demands, which compromises a fuller appreciation of the Anglo-Zionist partnership. On the other hand, Arab-Jewish relations in Palestine before 1914 are given adequate treatment.

Finally, the author alludes to an important yet little-understood ingredient of the wartime Anglo-Zionist combination that ensured the survival of the Zionist movement. Allied belief in the myth of an all-powerful international Jewry provided the rationale for using Zionism to promote Allied imperial interests in the Middle East (a myth that was also part of the substance of anti-Semitism). Vital correctly sees the irony that this myth attained its greatest acceptance just before it was tragically exposed for what it was.

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JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 5, *Christian Doctrine and Modern Culture (since 1700)*. Chicago: University of Chicago Press. 1989. Pp. xlix, 361. \$29.95.

This volume completes a project that is singular in modern scholarship. Nearly twenty years ago, the first volume of *The Christian Tradition* announced a study of a kind far more characteristic of the Enlightenment than of the late twentieth century. Unaided by collaborators and committees, one author had undertaken to review single-handedly the entire course of Christian doctrine in the lands of Eastern, as well as in those of Western, orthodoxies, from the post-Apostolic era to the present day. In the fifth volume, Jaroslav Pelikan concludes his monumental task, with a comradely glance toward Edward Gibbon.

Like all of the earlier books, his work devoted to doctrine since 1700 is characterized by a mastery of the sources of ecclesiastical history, an encyclopedic knowledge of scholarly literature, and a penetrating, yet graceful, manner. The particular subject of this volume is the crisis that historical criticism of Scripture and tradition brought about, with gathering momen-

tum, from the sixteenth century on. It discredited age-old claims to unalterable and unadulterated truth in doctrine; thereby, it overturned the legitimacy of claims to religious coercion exercised through organs of ecclesiastical and secular government. Pelikan accounts for the continuing, often anguished, reassessments by Christians of their inmost convictions, and he provides a topography of those reappraisals as they spread through all of the magisterial denominations.

The subtitle of the series indicates Pelikan's own stance on the central issue of whether Christian doctrine was an inviolate deposit, the same from the beginning and forever, or a motley creature of Christianity's experience in the world. And yet it is fair to say that this volume sets forth not the maelstroms out of which development came but the results; one of the greatest results was the discovery and eventual acceptance of the idea that doctrine was the cause, the subject, and the result of historical development.

Pelikan's account does not extend beyond the formal world of ideas to the circumstances in which ideas are put to the test. Thus, for example, one will not find considerations of how the Nazi cataclysm altered ideas of providence or ways in which Christians conceived their own calling in the world, nor will one find a study of the social tensions that eventually gave rise to Liberation Theology (a line of inquiry that falls beyond the chronological end of the book, which is the Second Vatican Council). Through the generations, such circumstances as these drove to ever-higher powers the reappraisals that historical criticism ignited. From the parables of Jesus on, the confrontation between scriptural ideals and human conduct had been engrained with the danger of hypocrisy, whether blatant or unrecognized. But historical criticism and events relentlessly assailed the truthfulness of the ideals in a new and radical way; they challenged the very goal of assimilating Scripture to life.

Even without discussing events, Pelikan subsumes their effects in his portrayal of a new search for spiritual integrity that bridges confessional differences, calms strife, and embraces "the whole of humanity." But it is clear in this openness how far short the churches still fall of the ideal of human nature conveyed to them through their common legacy. For they teach a humanity that cannot return to discord and corruption after it has been formed for the worship of God and has sung the incorruptible song of truth, one that, fitted to the lyre of life as the chords and strings, is able to resonate with Deity (see Methodius, "Fragment I: From the Homily on the Cross," in Alexander Roberts and James Donaldson, trans., *Ante-Nicene Christian Library*, vol. 14 [1869], p. 223). The ferment remaining even in a demythologized Christianity without Christ is evident in the irenic, valedictory credo of this great study; it underscores the character of faith as a strategy in a struggle for existence and poses the

question of whether, if there were no struggle, faith would have to invent one.

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CHARLES D. CASHDOLLAR. *The Transformation of Theology, 1830–1890: Positivism and Protestant Thought in Britain and America*. Princeton: Princeton University Press. 1989. Pp. xii, 489. \$35.00.

Charles D. Cashdollar makes two cardinal points in his thoroughly researched and sometimes unnecessarily detailed study. First, against any lingering notion that theology and scientifically oriented thought confronted each other during the nineteenth century in simple hostility, Cashdollar demonstrates their actual relationship to have been exceedingly complex. Second, he argues that the true pivot of the scientific-religious controversy of the time was not biological evolution but "positivism," a term that, despite its close original association with Auguste Comte, soon became a buzzword lending itself to loose usage and strong emotions.

On the second, more problematical point, Cashdollar makes a strong case that, for serious religious thinkers, Comte represented far more of a threat than Darwin. Except for biblical literalists, it was possible to see providence at work in evolution; rather than destroying the argument for God from design, according to a nurtured view, natural selection simply displayed a more profound and therefore greater design. But positivism, by limiting human knowledge to that of observable phenomena, called into question the entire validity of religious belief and doctrine. Indeed, it was largely because of their perceived association with Comtism, Cashdollar contends, that evolutionary ideas became suspect.

Questions naturally arise. Why, if positivism was the real issue, did evolution become so conspicuously the focus of public clamor? And why did the evolution controversy so notoriously outlast the debate over positivism, which, as Cashdollar notes, faded in the 1890s? Did guilt by association extend so far? A more plausible answer might distinguish between the epistemological concerns of intellectuals and popular fascination with the less abstruse questions of human genealogy and chronology. The author does not pursue this distinction, however, confining his attention very largely to articulate clergymen and theologians. This is entirely legitimate, but, if "Protestant thought" extends to less formally expressed attitudes, his conclusions may be misleading.

Cashdollar's diligence in exploring the spectrum of theological response to positivism can hardly be faulted, however. Embracing figures who range in prominence from John Henry Newman, Benjamin Jowett, and James McCosh to the now forgotten, his findings indicate a surprising degree of willingness to

seek accommodation with the new philosophy. The author's frank sympathy with this approach may lead him to give special emphasis to the "liberals" and "judicious conservatives" who best exemplify it, but his evidence is generally persuasive. Thus, while acknowledging that some Protestant thinkers retreated into the fortresses of fideism, ecclesiastical authority, or biblical inerrancy, Cashdollar shows that many others found in positivism an opportunity for the fruitful reinterpretation of doctrine.

This was possible because, despite Thomas Huxley's epigrammatic definition of Comtism as "Catholicism minus Christianity," positivists and Christians in fact shared some significant common ground, especially in matters of practical and social ethics. Altruism was commended by Comte as well as by Jesus, after all, and Cashdollar maintains that one of the most important results of positivism was to reinforce and heighten the churches' sense of social responsibility.

There were national differences, although Cashdollar does not delineate these as systematically as he might. He does suggest that Americans, perhaps because their views were more closely defined by denominational ties, were less drawn to extreme theological departures and more attracted to the middle ground than were the British. This moderation seems at times also to reflect an ingenuous optimism, as in the marked eagerness of American clergymen to find a Christian basis for the social sciences.

The nineteenth-century "crisis of faith" has been a subject notably liable to stereotype and oversimplification. In making us more completely aware of the manifold responses to positivist thinking, Cashdollar does much to clarify our understanding of it.

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WOLF LEPENIES. *Between Literature and Science: The Rise of Sociology*. Translated by R. J. HOLLINGDALE. (Ideas in Context.) New York: Cambridge University Press. 1988. Pp. viii, 388. Cloth \$49.50, paper \$15.95.

Three cultures, argues Wolf Lepenies in an ingenious reworking of C. P. Snow's famous observations on the "two cultures," have dominated scholarly discourse since the late eighteenth century. While science and literature have occupied the extremes of intellect and sensibility, the social sciences have taken up a distinctive, intermediary *juste milieu*. Sociology in particular has best fulfilled its aim of enlightening modern society, according to Lepenies, through polite but distanced conversation with the other cultures. This study impressively illustrates how sociology and literature (science recedes into the background after the introduction) interpenetrated France, England, and Germany from the early nineteenth to the mid-twentieth century. Their relationship has been far more continuous than one would have supposed, and Lepenies's

story has provocative implications for both the historical and the contemporary status of the social sciences.

Lepenies's narrative skims elegantly from encounter to encounter. We begin in France with chapters on Auguste Comte's cultic adoration of Clotilde de Vaux, an aspiring novelist, and Emile Durkheim's campaign for science and democracy against traditionalist and reactionary men and women of letters. The section on England treats John Stuart Mill's famous break with his father's excessively intellectual utilitarianism and his tutelage in feeling from romantic poetry and Harriet Taylor; Beatrice Webb's lifelong flirtation with imaginative writing and her and Sidney Webb's infatuation with Soviet socialism; H. G. Wells's attempt to capture sociology for the utopian novel; and the embarrassed but seemingly irresistible attraction to sociology of literary critics from Matthew Arnold to F. R. Leavis and T. S. Eliot. After a brief look at W. H. Riehl's ominously aestheticizing folk sociology, the slightly repetitive section on Germany eddies around the ambivalent relationship between the poet-cult leader Stefan George and his contemporaries Georg Simmel and Max Weber; Thomas Mann's admiration of Weber; Karl Mannheim's astringent repudiation of social romanticism; and the compromising embrace between the sociological expressionism of Hans Freyer and the Nazi regime. Sociology has seldom been a chaste partner of science because, as Lepenies reports, its meetings with literature have been irrepressibly frequent, occasionally foolhardy, and surprisingly fruitful.

Literary and sociological romance gives way to warfare in the book's second preoccupation, the conflict between Enlightenment and "counter-Enlightenment" for possession of sociology. Lepenies himself writes as an unabashed partisan of Enlightenment, which he identifies with the modern age against the *ancien régime*, revolution against restoration. These dichotomies distort sociology's highly ambiguous relationship to the Enlightenment. The discipline took shape for Comte and Mill as an assimilation of eighteenth-century thought to the changed conditions of the nineteenth. And in Germany, Ferdinand Tönnies (the founder of the German sociological tradition) and Weber were hostile critics of utilitarianism and engaged in their own complex mixtures of critique and appropriation of Enlightenment thought. The book's lack of sustained conceptual or textual analysis permits its rigid schematizing to go unexamined; crucial test cases such as Mill's *Autobiography* (1873) or Weber's *Protestant Ethic* (1904–05, 1920) were written, among other reasons, to contest precisely the kind of dualities it tries to revive. The effect of introducing this second theme is an unevenness in the book's tone, which wavers between playful irony when it indulges Lepenies's own erudite enthusiasm for literature and ideological prudery when he remembers that literature tempts us away from sober sociological analysis.

Although he does not directly address current intellectual fashion, Lepenies implicitly takes issue with the postmodern conflation of literature and learning. In

the context of his book, the subversion of social science by rhetoric and the deriding of disciplinary expertise no longer originate with Jacques Derrida, Michel Foucault, and Jean-François Lyotard; rather, these names fit established conventions in the long history of literature's attacks on modern social analysis. Lepenies accuses this continuing revolt against sociological reason of an unchecked aestheticization of politics, which in the twentieth century furthered fascination with the Nazi and Stalinist regimes and naiveté about their consequences. A cosmopolitan master of his subject, Lepenies argues for an ultimate separation, despite their frequent intercourse, of letters and science, art and politics. Although it poses their historical differences too sharply, his book powerfully counters the current vogue for homogenizing them. Its plea for a distinctive sociological culture deserves a wide audience among readers intrigued by the latest round of dangerous liaisons between the disciplines.

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DENNIS L. SEPPER. *Goethe Contra Newton: Polemics and the Project for a New Science of Color*. New York: Cambridge University Press. 1988. Pp. xvi, 222. \$39.50.

Every book is autobiographical, wrote Samuel Butler in *The Way of All Flesh*, a point certainly true of Goethe's *Theory of Colors* (*Farbenlehre*), a major topic of Dennis L. Sepper's book. Sepper's volume does not contain the paramount information that the origins of Goethe's *Theory* go far beyond the moment when, in 1790, Goethe had for the first time a prism in his hand and performed an amateurish experiment. Almost twenty years earlier a very amateurish book on science fell into Goethe's hands and heavily colored his views on science for the rest of his life.

The book was *Le Système de la nature* by Baron d'Holbach, a leading exponent, with Julien O. de la Mettrie, of a crudely mechanistic and materialistic view of nature as containing only matter and motion. Not knowing, contrary to Sepper, anything but superficialities about Isaac Newton's science, young Goethe took it to be equivalent to a "melancholy atheistical halfnight." Goethe's revulsion, and that of his equally young and unscientific friends, knew no bounds. They, in the description in Goethe's intellectual autobiography, *Dichtung und Wahrheit*, threw themselves "into living knowledge, experience, action, and poetising, with all the more liveliness and passion."

Two decades or so later that revulsion suddenly turned, under the impact of Goethe's chance encounter with a prism, into a crusade that he waged, for the rest of his life, against Newton's science of colors. "As for what I have done as a poet I take no pride in it whatever. . . . But that in my century I am the only person who knows the truth in the difficult science of

colors—of that, I say, I am not a little proud, and here I have consciousness of superiority to many.”

This statement of Goethe, recorded by Johann Peter Eckermann, is not quoted by Sepper. He also glosses over the many crude invectives penned by Goethe against Newton and other physicists. Obviously, those texts can make suspicious any judicious reader of a book whose immediate aim is to restore intellectual respectability to Goethe’s “science” of colors. Sepper’s ulterior aim is to plead on behalf of a new and more complete understanding of what physics is about.

The first aim is dealt with in the first three chapters. There Sepper defines the questions of Goethe’s *Theory*. He analyzes the origins of the *Theory* before he surveys the problems besetting Newton’s theory of white light and colors. The ulterior aim is articulated in the last two chapters. One is on the factuality, certainty, and organization of science. The other is even more revealing in its title, which is “Goethe and the Ethos of Science,” where the last section deals with “*aperçus*, language, and the problematics of truth.”

Compared with important earlier studies, nothing essentially new is offered in the first three chapters as far as historical material is concerned. The novelty, of an essentially philosophical character as far as interpretations of Goethe are concerned, is contained in the last two chapters. There much is made of the praises accorded by Werner Heisenberg in 1941 to Goethe the scientist. Heisenberg took Goethe for an early advocate of the principle of complementarity as the basis of a new and broader meaning of physics in which exact (strictly quantitative) and nonexact (philosophical, nay, poetical and mystical) considerations may play equally important roles.

Sepper ignores the fact that this “broader” view of physics was reached by Heisenberg years before he formulated the principle of indeterminacy in 1927. It was as an enthusiastic member of the very romantic *Jugendbewegung* that Heisenberg first parted with the idea of causality. In his case, too, as in countless other cases in the history of physics, the philosophy had already been on hand before it was furtively grafted onto some mathematical formalism.

Physics has, in terms of its mathematical structure, a life independent of its fashionable philosophical interpretations, regardless of whether these are marketed by physicists, often amateurish in matters philosophical, or by professional philosophers of science, all too often with no expertise in physics. Only by ignoring this elementary fact about the intellectual history of physics will one take too seriously, as does Sepper, the views of Karl Popper, Paul Feyerabend, Imre Lakatos, and Thomas Kuhn on the “irrational” components of scientific thinking. Only through the same oversight will one be tempted to do something even worse, namely, to see in modern physics, as anticipated by Goethe, a vote on behalf of Eastern mysticism.

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JOAN JACOBS BRUMBERG. *Fasting Girls: The Emergence of Anorexia Nervosa as a Modern Disease*. Cambridge: Harvard University Press. 1988. Pp. 366. \$25.00.

During the past fifteen years, the interactions between women, medicine, and culture have become a familiar topic in historical literature. Focusing on Western countries, scholars began listening to an age-old conversation in which women, physicians, and social ideologues discussed the character of female health and sickness. Initially, doctors dominated the conversation, because early studies concentrated on medical theories and treatment of women’s ailments. Historians next sifted through popular and prescriptive literature to uncover social attitudes about female health in various periods. Gradually, women’s own voices entered the conversation, as scholars considered the experiences of female patients, physicians, nurses, and health reformers in the United States and elsewhere. Thus, the historical transcript of the conversation is becoming more complete and more dynamic.

Joan Jacobs Brumberg’s study of anorexia nervosa among adolescent girls during the past century is an impressive reconstruction of a particular medical dialogue. In eating disorders, a woman’s body and appetite become her voice; food is a “symbolic language” (p. 2) through which women, doctors, and culture address each other.

The phenomenon of female fasting actually predates today’s “epidemic” of anorexia nervosa. Prior to modern times, however, the language of food restriction had religious, rather than medical, overtones. During the Middle Ages and Renaissance, prolonged abstinence was an acceptable, even revered, expression of female holiness. In the seventeenth and eighteenth centuries, scientific rationalism began to label the behavior a physical symptom that required an organic explanation. During the last quarter of the nineteenth century, Brumberg establishes, doctors classified anorexia nervosa as a distinct clinical condition that affected a specific population. Restricted eating became a pathological, not a pious, act, and religious voices gave way to secular and medical discourse.

Why did anorexia nervosa first appear in the modern era, and why have most anorectics been young, white, middle- and upper-class females in Western countries? Three different explanations—biomedical, psychological, and cultural—have emerged. In her concise critique of those models, Brumberg underscores the need for a multidimensional (rather than reductionist) interpretation of eating disorders. Without discounting biological factors, she argues that “disease is a cultural artifact, defined and redefined over time” (p. 3). Because historical parameters shape the incidence, symptomatology, and meaning of illness, “[c]ulture is the critical variable” for understanding the language of anorexia nervosa among doctors and adolescent girls (p. 7).

Brumberg listens to the medical voice through doctors’ publications, private papers, and institutional rec-

ords. Against the backdrop of professionalization and social change, Brumberg traces how the diagnosis, interpretation, and treatment of anorexia nervosa evolved during the late nineteenth and early twentieth centuries. In the 1870s, the work of "public asylum keepers in the United States," "elite British medical consultants," and "early French psychiatrists" (p. 102) led to the identification of anorexia nervosa as a neurological disorder, making it the province of specialists rather than of general practitioners. With little attention to etiology, neurologists attempted to reduce the anorectic's most prominent symptoms. Following social norms of femininity, they hoped to transform the petulant, self-centered faster into "a nice, plump, good natured little girl" (p. 122). In the twentieth century, scientific research and specialization prompted two new models of anorexia nervosa: it was an endocrinologic condition that required chemical therapy, or it stemmed from repressed emotions and sexuality, which only psychological intervention could cure. Both explanations reflected cultural assumptions: that anatomy is destiny (in this case, hormones ruled the female body) and that women's psychosexual nature is basically flawed. Although the differences between American, English, and French medicine are not explored sufficiently, Brumberg does demonstrate how intellectual, professional, and social context framed the language in which doctors spoke about anorexia nervosa.

The other voice in the conversation, that of the anorectic herself, is emphatic: her ritualized eating and emaciated body are blunt public statements. Yet the patient's voice is also muffled, because, until recently, neither doctors nor anorectics recorded it. Because of that historical silence, Brumberg avoids theorizing about the individual psychology of anorectics and, instead, deciphers their voices by means of social history and women's studies.

In discussing the late nineteenth century, Brumberg's focus is the psychodynamics of the middle-class family. The bourgeois Victorian family was intense and intimate; structured around love (rather than overt discipline), it prescribed dependence and ultimately marriage for girls and allowed them little privacy or autonomy. Because emotional displays were forbidden, members of bourgeois families spoke indirectly; the offering, acceptance, or refusal of food was a coded language through which parents and children asserted themselves. That was especially true for mothers and daughters: Victorian culture declared that food preparation was a maternal duty, but that indulgence of any sort was unfeminine and immoral. Brumberg's translation of the family's (and mother's) voice is insightful; her rendering of the anorectic's "unhappiness" (p. 138) and "emotional hostility" (p. 140) is suggestive.

In the twentieth century, Brumberg's attention shifts from the microcosm of family conflicts to the macrocosm of cultural pressures. She describes how, to this day, Western society continues to define and evaluate women through their bodies. The modern equation between thinness, beauty, and success had many

sources in the early 1900s: the standardization of weight charts, the fashion industry and the production of sized, ready-to-wear clothing, the work of pediatricians and home economists, and changing norms of heterosexual attractiveness and relationships. By the 1920s and 1930s, social convention was clear: femininity and appetite are incompatible; weight is the measure of a woman's worth and character. By the late 1940s, adolescent girls constituted the market for those rules. By the 1970s, today's hyperactive, competitive anorectic had emerged.

Brumberg's themes will be accessible and thought-provoking to a variety of audiences, including social historians of medicine, students and scholars in women's studies, and health professionals as well as lay readers. Above all, she explores modern culture's attempt to limit the vocabulary by which women communicate: they should express their identity through their bodies. Throughout Brumberg's book, a loud and insistent chorus tells adolescent girls what to do and who to be. Doctors, family members, and virtual strangers almost drown out the anorectic's own voice. Though present, she rarely speaks directly to the reader about her private struggle.

Ironically, that feature of the book reenacts a critical dynamic of eating disorders. Although their abilities and privileges are numerous, anorectics and bulimics are impoverished in one profound sense: they distrust their own needs and strengths. They learn to be quiet, and the world forgets how to listen. Searching for an audible voice, the anorectic does precisely what is asked of a woman: in the nineteenth century, she apparently suppressed all forms of pleasure; in the twentieth century, she pursues thinness with individualistic fervor. Yet "perfection" evokes only censure; it does not bring the anorectic any closer to being truly heard.

That helps explain the "denial and anger" (p. 3) that, for centuries, have been the emotive sources of female abstinence. "Discipline" and "sacrifice" are the words that Western society makes available to women. When an anorectic masters that vocabulary, though, she is labeled sick; more tragically, she loses her own voice, her authentic identity. In that abandonment of self, we mistakenly see "the best little girl in the world," the model of compliance and denial. As Brumberg implies, however, the anorectic's exaggerated body language expresses the obverse as well; it resounds with anger and desire. Perhaps it is time to recast the question: instead of wondering what women are denying, we should ask what they are hungering for.

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RALPH C. HANCOCK. *Calvin and the Foundations of Modern Politics*. Ithaca: Cornell University Press. 1989. Pp. xvii, 221. \$26.50.

Ralph C. Hancock bravely attempts a subject that should interest historians, theologians, and political

scientists. His book, after an extended introduction, contains two parts that address John Calvin's thought and a conclusion that explores recent discussion among political scientists about the origins of modernity.

Most of Hancock's introduction is devoted to presenting and criticizing how earlier historians of political thought, notably J. W. Allen, Quentin Skinner, and Michael Walzer, have analyzed and assessed Calvin's contribution to modern political theory. The bulk of the first major part explores the political ideas in Calvin's *Institutes of the Christian Religion*, especially the difficult last chapter (IV, 20) on civil government. Quoting liberally from many parts of the *Institutes* to illuminate Calvin's meaning, Hancock discusses the relation of religion and politics, Christian freedom and spiritual government, forms of government, authority and resistance (notably the right and duty of inferior magistrates to resist tyrannical monarchs), law and ethics, fallen reason and conscience. This section seems the strongest part of Hancock's work. He has read the text closely, and his writing is thoughtful and dense and sometimes shows a fine turn of phrase, although he is too fond of paradox. The second part, "Calvin's Antitheology: Transcendence without Another World," tries to relate Calvin's thought on politics to his theology of justification, sanctification, and man as the image of God. Here I felt Hancock's treatment was more elaborate than his purpose required. His conclusions try to rehabilitate but soften and refine Emile Doumergue's argument that Calvin was one of the founders of the modern world. To this end Hancock enters a prolonged critique of the theories of modernity developed by Leo Strauss, Karl Löwith, Eric Voegelin, and Hans Blumenberg, especially the tendency to equate modernity with secularism.

This book will please theologians and political scientists more than historians. Hancock writes a sort of abstract intellectual history that is no longer acceptable. There is nothing on excommunication, the problem that long bedeviled Calvin's relations with the Genevan government. There is nothing on Calvin's training as a lawyer and how he used it in the service of the Genevan government. Although there are detailed discussions of how recent political scientists understand Calvin, I noted references to only five contemporaries of Calvin. Under "B" in the index, for instance, there are no references to Theodore Beza, Martin Bucer, Heinrich Bullinger, Basel, or Bern. Indeed, I cannot recall Geneva itself being mentioned in the text. Can one understand Calvin without a historical context? His *Institutes* is also treated in a vacuum. Hancock ignores Calvin's letters, which often deal with concrete political problems, such as the persecution of French Calvinists. Calvin's Old Testament commentaries often touch political questions, but Hancock cites only a couple of secondhand references from Doumergue's biography. One cannot fault Hancock for not using William Bouwsma's recent *John Calvin* (1988), but Hancock could have profited from various works of André Bièler, Josef Boháček, Alexandre Ganoczy, Robert

Kingdon, William Monter, T. H. L. Parker, Charles Partee, and Suzanne Selinger.

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STEPHEN C. YEAZELL. *From Medieval Group Litigation to the Modern Class Action*. New Haven: Yale University Press. 1987. Pp. x, 306. \$33.50.

Stephen C. Yeazell's work traverses a vast chronological canvas, tracing the evolution of group litigation from the ecclesiastical courts of Canterbury in 1199 to the 1966 redaction of Federal Rule of Civil Procedure 23, which governs class actions in the federal courts. The book is essentially a narrative that breaks the history of Anglo-American group litigation into three periods: medieval, early modern, and modern. According to Yeazell, medieval law courts routinely entertained litigation involving diverse social groups without recognizing anything remarkable about treating unincorporated groups as litigative entities. The author found no medieval doctrine to explain or justify group litigation. During the sixteenth and seventeenth centuries, in contrast, the variety of group litigation shrank, the phenomenon itself began to be challenged, and the chancellors began to grope for a theoretical justification for permitting unincorporated groups to litigate. The answer lay in the idea of representation, which according to Yeazell was just creeping into the English consciousness. Finally, in the modern era, group litigation began to require elaborate justification because modern Anglo-American legal culture rests on individualistic assumptions rather than collective ones. As a result, group litigation lay nearly completely dormant during the nineteenth and early twentieth centuries until it was resuscitated in the guise of the class action. Yeazell's work makes a contribution to the literature of legal history by tracing the evolution of legal doctrines surrounding group litigation and, perhaps more importantly, by attempting to underscore the connection between this relatively narrow area of procedural law and broader social realities and ideas about representation.

At the intersection of judicial procedure and social and intellectual history, however, the author's work falls short of what he set out to accomplish. Yeazell painstakingly unearthed the legal doctrine concerning group litigation from case reports spanning eight centuries, but he does not do a convincing job of fitting the evidence from the case reports into the context of social and intellectual history. In part his failure results from reliance on some dubious secondary authorities. For example, Yeazell reiterates Susan Reynolds's assertion that "no system of medieval law developed any concept of legal personality or corporation before 1300" (p. 74). This statement would have dumbfounded the canonists of the twelfth century, who described the *universitas* or corporation as a *persona ficta*. Yeazell's reliance on Reynolds reflects unawareness of a rich

historical literature with continental roots. Like many lawyers before him, Yeazell disdains to recognize the existence of any ideas or institutions except those that appeared in the English case records. Because group litigation was in large part a phenomenon of ecclesiastical courts and chancery courts, both of which relied on the jurisprudence of Roman and canon law, and since ideas about representation, incorporation, and agency loomed large in the discourse of civilians and canonists before 1200, Yeazell's preoccupation exclusively with sparse English records impoverishes his analysis. In discussing group litigation in medieval, early modern, and more recent legal doctrine, Yeazell recognizes the vital importance of ideas about representation, but he fails to refer to a single thread of influence outside the Anglo-American tradition.

This book is a useful guide to the procedural ancestry of the modern class action. It is not a useful analysis of the social and intellectual history of groups, representation, and litigation.

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RONDO CAMERON. *A Concise Economic History of the World: From Paleolithic Times to the Present*. New York: Oxford University Press. 1989. Pp. xiv, 437. \$39.95.

The one-volume economic history of Europe from earliest times to the present, which seemed doomed to extinction, is making a comeback. The recent stimulating effort by Nathan Rosenberg and L. E. Birdzell, Jr., *How the West Grew Rich: The Economic Transformation of the Industrial World* (1986), was a milestone in this resurgence, as I noted in these pages (*AHR*, 91 [1987]: 96-97). Now Rondo Cameron has offered another more systematic survey, which he modestly describes as "a textbook for an upper division undergraduate course in European economic history" (p. vii). Well known for his seminal work on French economic development in the nineteenth century and for comparative studies in the history of banking and longtime editor of the *Journal of Economic History*, Cameron is well positioned to summarize and interpret the scholarly work of a generation.

An economist by training, Cameron follows a clear historical organization and has a historian's appreciation for factual accuracy. Beginning with an introductory chapter that outlines some basic economic concepts with admirable clarity, Cameron treats in successive chapters antiquity, medieval Europe, the non-Western world before 1500, European overseas expansion, and mercantilism. These chapters focus on well-established problems in economic history—for example, the rise of medieval towns, the price revolution, Spanish economic decline, agriculture and industrial technology, and the use of standard historical methods—while drawing gracefully on economic concepts

as needed. Turning to the era of industrialization in the remaining sixty percent of his work, Cameron provides in two interrelated chapters a general conceptual overview of modern economic growth and a dynamic historical account (to 1914) of its main determinants, population, resources, technological change, and social institutions. Case studies of industrial development in all European nations (plus the United States and Japan) compose two more chapters, as do the world economy (including imperialism) and three "strategic sectors" in the nineteenth century: agriculture, banking, and the state. The twentieth century is treated with an overview and two narrative chapters on the interwar and postwar eras.

Cameron has performed his ambitious task well. The writing is economical, precise, and eminently understandable. Differences in interpretation and major historical debates are noted and evaluated but without overwhelming the author's own judicious interpretations. Graduate students (and nonspecialist professors) will find this a most valuable guide and reference work. The bibliography is exceptionally well selected and annotated, for Cameron knows the literature and evaluates it with a judiciousness that infuses the entire book. There is also an abundance of illustrations, useful for the student and rare in general economic histories.

Certain limitations may also be noted. The "world" in the book's title is largely a misnomer, although there is enough non-European material to make it understandable. The chapters on the Middle Ages and the early modern period seem to parallel older treatments, with a heavy stress on political factors and government policy in explaining regional and national variations in economic performance. The chapters on nineteenth-century industrialization are the best in the book. The general stress is on the gradual, nonrevolutionary chapter of industrialization and the wealth of alternatives open to different countries in their march toward the industrial world. This stress grows out of Cameron's own work and reinterpretations, and it generally leads to more balanced judgments of the different national experiences. For example, Cameron effectively presents the revisionist view of generally good French economic performance in the nineteenth century, which he has done much to spread.

Cameron's strong opposition to the whole idea of an industrial revolution in Great Britain (or anywhere else) on the grounds that economic change was gradual and spotty, however, seems overdone. The question is complex and at least partly semantic, but it should be pointed out that Cameron's emphasis on technological change and "epochal innovation" (p. 195) in fueling long-term economic development actually fits rather well with the original concept of the industrial revolution as an interrelated cluster of revolutionary technological breakthroughs, of which the new "factory system" was the overarching expression and summation. The fact that macroeconomic rates of national economic growth moved up gradually and that agricul-

tural and artisanal production long remained dominant in quantitative terms misses the epoch-making quality of the radical technological discontinuity in certain key industries, most notably cotton textiles, metallurgy, and power. That such major questions come alive and force the specialist to think is, of course, a tribute to this fine survey of a field that has badly needed such a magisterial updating and reexamination. Cameron's work is now clearly the best of its kind.

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PAUL BAIROCH. *Cities and Economic Development: From the Dawn of History to the Present*. Translated by CHRISTOPHER BRAIDER. Chicago: University of Chicago Press. 1988. Pp. xxii, 574. \$49.95.

For Paul Bairoch, urban life is both natural and implausible in human society. He is always mindful that the urban dweller is vulnerable, not to say helpless, unable to grow the food and gather the fuel necessary for life. On the other hand, whatever level of urbanization the state of economic development makes possible in a given historical situation will, perhaps with a lag, materialize. No particular external impulse is required, although trade, conquest, and religious fervor may nucleate the agglomerative process. Moreover, cities, once established, endure much or tend to recover from adversity. This positive reading of history has much to recommend it, but, like other fruitful points of view, it can, and here sometimes does, tempt one to questionable extrapolation.

This book brings to English-language readers the ambitious synthesis first published three years earlier as *De Jéricho à Mexico*. Both titles yield a bit to literary license: treating more than cities, let alone particular cities, the book surveys urbanization over a great span of time and space. This version brings up to date some statistics from 1980 to 1985, and it is generally quite serviceable despite a few questionable or confusing translations.

The author measures urbanization by the share of population living in towns and also tracks the number of big cities and their growth and share in the urban population to flesh out the quantitative picture. Vital rates, notably infant mortality, not only help account for urban growth but also indicate the quality of urban life and thus proxy the costs of urbanization. Such qualitative touches figure comparatively little in this book, despite asides that make clear the author's personal concern for cities both as artifacts of civilization and as human communities. Here he is essentially the social arithmetician, tracking and balancing energy and economic activity as well as people. Even the work life of townspeople receives sketchy treatment: cities house merchants, artisans, factory and service workers, yet their productivity seems to matter less to urbanization than that of farmers.

Even within his self-imposed limits, Bairoch rather slights such topics as urban systems, migration, and the distinctive nature of industrial conurbations. On the other hand, the steady focus on farm labor productivity and yields on land strongly ties together otherwise disparate experiences across the miles and centuries. Thus, the hyperurbanization of today's Third World, which Bairoch struggles not to label hopeless, has engendered "Romes without empires" (p. 597). Agricultural progress must now rescue the swelling cities just as it nursed them into being and then launched their conquest of the industrializing world.

The central argument, then, builds from agricultural surplus. Repeatedly Bairoch works to develop this insight into a full-fledged positive-feedback model of urbanization and development—in which cities breed civilization and innovation, hence, higher productivity and further urbanization—but then he pulls back. Not only can urbanization lag behind agriculture, or in much of today's world outpace it, but the positive effects of urbanization on development prove remarkably elusive. The quest for a logical and consistent account leads Bairoch to touch on many economic issues and debates; every chapter has headings in the form of a question. I find his argument much more persuasive and original when the data come into play than when he uses concepts taken from what passes for the theory of development. With statistical data he becomes a numerate virtuoso, cajoling, cautioning, combining, comparing until the reader is likely to surrender any critical reservations.

The statistics are central to what is strongest in this book, and many are the author's own (a largely bilingual companion volume, *The Population of European Cities, 800–1850* [1988], sets out the data base and some methods and sources for the core region and period). Yet one sometimes does not know what to make of them. Let me focus on Europe at the millennium. About 12 percent of the population of Christian Europe west of Russia supposedly lived in towns around 1000 (as indeed seven hundred years later). Uncertainty aside, this estimate by Bairoch calls for more drastic historical reassessment than is hinted at here. "Dark ages" or not, this was a society of autarkic manors and embattled monasteries fortified against endemic violence from nearby as well as distant scourges. Where is the high-volume trade and steady agricultural supply required by four million town dwellers? What record have these people left, especially outside Italy?

In leveling this criticism at Bairoch's enormous achievement, I am simply arguing that qualitative plausibility can serve as a check against numbers run wild, just as even crude numerical data can control the potential excesses of verbal stereotypes and imaginative narrative.

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CHRISTOPHER THORNE. *Border Crossings: Studies in International History*. New York: Basil Blackwell, in association with the East Asian Institute, Columbia University. 1988. Pp. x, 313. \$55.00.

During the last two decades, Christopher Thorne has produced some of the most interesting and influential studies of international relations in the twentieth century. Three of the best-known books, *The Limits of Foreign Policy: The West, the League, and the Far Eastern Crisis of 1931-33* (1972); *Allies of a Kind: The United States, Britain, and the War Against Japan, 1941-45* (1978); and *The Issue of War: States, Societies, and the Far Eastern Conflict of 1941-45* (1985), have focused on the ways in which Britain and the United States responded to Japanese imperialism and emergent Asian nationalism. Diplomatic behavior, Thorne has argued, must be understood as a product of cultural forces and social structures, not as some isolated imperative. A major subtheme of all of Thorne's work has been his interest in exploring the implications of the transfer of economic, political, and social power from Great Britain to the United States during the twentieth century. This spirited collection of essays emphasizes these themes.

The volume includes a dozen sketches, all of which appeared previously as scholarly articles or lectures. The author has left them intact, adding only citations to recently published work by other scholars. The greatest virtue of the collection is to bring together in a single volume essays that have appeared in scattered and occasionally obscure publications.

Thorne has often criticized "traditional" diplomatic historians of doing little more than chasing down obscure foreign office records. In the process, he complains, they ignore the domestic cultural and social context of international relations. He has urged diplomatic historians to use methods developed in such allied disciplines as political science and sociology. In several of the essays included here, Thorne repeats the call to produce what he refers to as "international history."

Despite this injunction, it is not clear that the author has followed much of his own advice. This comment is not intended as a criticism, however. Thorne's work is powerful not because he has found a social science methodology ignored by other historians of foreign relations but because he writes superbly, engages in an awesome amount of research in public and private archives of many countries, and brings to his scholarship a keen appreciation of irony and literary grace. He makes his reader feel deeply about the issues under discussion and enriches the enterprise through constant glimpses into the processes by which British, American, Japanese, Australian, and other policy makers grappled with clashing national interests.

The essays focus on the diplomatic and military crises of the 1930s and 1940s but explain these developments in terms of much larger economic, political, and cultural trends. For example, a piece titled "Indochina and Anglo-American Relations, 1942-45,"

touches on such diverse questions as the relationship between British and French imperialism, traditional Vietnamese-Chinese rivalries, the precipitous decline of British influence in Asia vis-à-vis the United States, and the impact of Soviet-American hostility on America's emerging colonial policies. In the process, Thorne reveals the layers of complexity that historians must assess in unraveling what at first seems merely a technical question regarding Washington's attitude toward the restoration of French authority in a relatively obscure colony. Similarly, an essay based on the private journal of a British officer in the southwest Pacific theater provides startling insights into the relationship between Australia, Britain, and the United States during World War II. In other sections, Thorne examines how British views of black G.I.'s provide insight into the larger question of the racial aspects of the world war and how wartime developments still shape American society. Several essays analyze the doomed efforts during the 1930s to find methods, short of war, to contain Japanese power.

Methodologically speaking, Thorne may not have succeeded in identifying or practicing the synthesis of social science theory that he believes critical to reviving diplomatic history. Nevertheless, this collection presents a delectable sample of some of the most thoughtful work in the history of international relations.

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MICHAEL MANDELBAUM. *The Fate of Nations: The Search for National Security in the Nineteenth and Twentieth Centuries*. New York: Cambridge University Press. 1988. Pp. xi, 416. Cloth \$39.50, paper \$13.95.

This book is a tour de force. Michael Mandelbaum, an authority on the impact of the nuclear weapon on the contemporary world, has produced a synoptic history of the nineteenth and twentieth centuries through the experiences of six developed nations with the problem of national security. Given the anarchic state of the international system, the search for national security is a vital element in the history of the nation state, whether it is pursued by individual efforts or by collective actions. Anarchy is defined not as chaos but as the absence of an international authority to protect states from one another. The author's assumption is that the security of any nation is primarily determined by its relation to other nations, not by any quantitative measurement of its economy or military arsenal.

Mandelbaum has built his history on the records of nineteenth-century Great Britain, France between the two world wars, and the United States, China, Israel, and Japan in the post-World War II era. He separates the nations into three pairs, with each representing similar security policies. The first pair, Great Britain and Japan, centers on their collective approach to national security. The second pair, France and Israel, deals with issues facing nations roughly equal in

strength in the international system. In his discussion of the third pair, the United States and China, Mandelbaum analyzes countries either very strong or very weak.

Some of these pairs make odd couples. Israel, a middle power like France, has felt itself under siege throughout the period discussed, that is, throughout the entire history of the State of Israel. Unlike France, it has had few options beyond a military build-up. The link between Israel and France in Mandelbaum's schema is their common membership in the large middle category of nations roughly comparable in strength to their enemies.

The concept of national security for the United States as a great power and China as a weak power is easier to categorize than it is for the middle group of nations. Unlike the middle powers, where domestic divisions are played out in a variety of approaches to national security, the very strong and very weak have their behavior determined by their position in the international system.

Mandelbaum's interest is centered on Great Britain and Japan, two nations that benefited from collective security in the form of a managed balance-of-power system. The Concert of Europe was never perfect, but it permitted the British to enjoy global power at minimal cost. After World War II, Japan's power was not imperial as Britain's had been; it was economic and the consequence of a managed equilibrium in Asia assured by American support. Mandelbaum likens the collective approach to national security to a cartel working in an international market. For mutual security, or mutual profit, the members modify their normally competitive behavior. Britain in the nineteenth century is identified as a "fringe firm" benefiting from the oligopolistic price without having to sacrifice sales or market shares. Similarly, Japan benefited from a liberal economic order after World War II without having to pay the costs of military preparations.

The author has made no claims of original research in preparing this book. He has relied on a wide variety of the best secondary works that he could find. It is an impressive effort. His footnotes are filled with useful quotations, alongside his own comments. He notes on one occasion that "Italy also considered itself a great power, and the others sometimes treated it as one" (p. 129). In a lengthy footnote he presents the Arab case against Israel while admitting his lack of sympathy with it (p. 272).

Although this is a learned and often-exciting book, historians may be discomfited by Mandelbaum's approach. His interest in the histories of the six nations is too often shaped by the procrustean beds that he makes for them. If his categories are too sharply defined, his definition of national security by contrast is too vague. He does not define the concept until the beginning of his last chapter (p. 329). All things considered, the book is worth reading for its insight and

information rather than for its application of national approaches to security.

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WILLIAM R. ROCK. *Chamberlain and Roosevelt: British Foreign Policy and the United States, 1937-1940*. Columbus: Ohio State University Press. 1988. Pp. xiv, 330. \$30.00.

To what extent are individual leaders responsible for policy and its manner of execution? In this study, William R. Rock thrusts onto Neville Chamberlain's and, to a lesser extent, Franklin Roosevelt's shoulders responsibility for the course of Anglo-American relations in the late 1930s.

Rock emphasizes the "legacy of suspicion in both economic and political relations" (p. 4) that conditioned Anglo-American relations by 1937. Both countries wanted to prevent—or alternatively, in the case of the United States, to stay out of—a war, but their leaders had diminishing reserves of sympathy for each other's style and approach. Chamberlain responded to the warnings of the chiefs of staff that Britain could not fight simultaneously three enemies in three separate theaters of war and that one at least should be pacified; consequently, according to Rock, he was more concerned with eliminating a possible enemy, Italy, than in acquiring a possible ally, the United States. Hence, he followed the policy of appeasement, at the beginning an honorable plan but one that Chamberlain held to against growing evidence that it was ineffective and, indeed, counterproductive.

Roosevelt, that supreme pragmatist, supported Chamberlain's moves to the extent that, first, he knew what they were, and, second, they seemed to be working. But Munich rather sickened him, and he refused the public approbation desired by Chamberlain. He had also by that time been rebuffed by the prime minister in his attempt to call a conference to further peace. It is not difficult to understand Chamberlain's indifference toward this traditional American approach to world crisis, but it is worth noting that it was Roosevelt rather than Chamberlain who first understood that Germany would fight and not be deterred by "a policy of conciliation that was tempered by firmness" (p. 11). Nevertheless, the earlier rebuff seems to have dampened any inclination Roosevelt might have had to take on the American isolationists publicly and fight to help Britain; the prime minister, after all, had made it clear that U.S. help was neither required nor desired.

This approach to Chamberlain is one way that Rock's book is different from David Reynolds's *Creation of the Anglo-American Alliance, 1937-41* (1981). Both emphasize Chamberlain's dislike of and distaste for the United States, but Reynolds argues that Chamberlain wanted at least the appearance, if not the reality, of American support. Rock contends that Chamberlain frequently did not even want the appearance of U.S. involvement: Americans talked rather than acted, and,

when they did act, they extracted a price. On the whole, Chamberlain believed that eliminating the United States from his calculations was a realistic option. Beyond this difference, Rock's book covers a shorter time period and is altogether less comprehensive in its coverage.

Rock's focus is political relations but with a consideration of small issues as well as large, giving the reader a slight whiff of the card index. Rock is gracious in his recognition of earlier work, and the result for specialists is a certain familiarity in the material. The book should be read for its interpretation, not for its revelations. For those whose main concern is Anglo-American political relations, however, it is a useful and well-written work.

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ANCIENT

GIOVANNI GARBINI. *History and Ideology in Ancient Israel*. Translated by JOHN BOWDEN. New York: Crossroad. 1988. Pp. xvi, 222.

This reworking of Giovanni Garbini's articles on biblical topics is intended "to investigate how far [the Old Testament] . . . is history and how far . . . ideology." It serves also to relieve the author's anger at Old Testament scholars' neglect of the findings and theories of Semitic linguists (including himself). The most offensive offenders, because the most influential, have been German scholars, but Garbini's hostilities are ecumenical. The modern misinterpretations of the *Alttestamentler* are matched by the ancient misrepresentations of "the Jerusalem hierocracy, shut up in a city which seemed to have lost all political authority, . . . withdrawing into their own past, typical of cultures in decadence," and described in anticlerical polemic that suggests contemporary analogues.

Garbini's book thus presents us with the same problem he found in the Bible, that of distinguishing history from "ideology," for example, propaganda. Fortunately, he tells us how he came to undertake the task. "In 1980 . . . trying to understand . . . the episode of the Golden Calf . . . I was struck by the detail that the tables of the law were written on two surfaces . . . and were broken so easily by Moses." Therefore, they must have been not stone, although the text says they were, but terracotta, like "cuneiform tablets" that "had disappeared from . . . Palestine by the end of the XIII c. B.C." Therefore, "the story . . . written by someone . . . familiar with such tablets, must have been composed" in or after the Babylonian exile. The whole story was therefore exilic propaganda.

This great discovery not only started him on his search but also indicated its goal and exemplified the methods to be used. Later, on page 104, he returns to this initial insight and insists that "in the ancient near east there were no fairly small stone tablets written on

both sides and containing a fairly long text like . . . the decalogue." By amazing logic (p. 105) "that means that the whole story was thought up in Babylon."

Semitic linguists commonly neglect Egypt, but the Bible says the Israelites had been there, and in 1922 the British Museum had at least ten small limestone tablets inscribed, some on both sides, with texts, dated mostly from 1600 to 1100 B.C., several of them evidently longer than the 172 words of the Exodus decalogue. Breaks are not mentioned but probable. The bottom is broken from a smaller, tenth-century tablet found at Gezer. Even a child can break a thin plaque of limestone; Lucian did so when a child and got a whipping that turned him from stonecutting to literature. *O felix culpa!*

Even if such limestone scratch boards had not been found, the device is so easily inventible that their failure to occur in the results of chance finds and spotty excavations would have been almost insignificant. So little of what has been recovered that arguments from archaeological silence, against likely conjectures, are hardly worth publishing. Unfortunately, Garbini uses them often. He is also fond of using a late interpolation to date the whole of the text in which the interpolation stands and of taking the contradictions between conflated texts, or between editorial additions and an original text, as proof that the whole resultant text is worthless as evidence of what the original reported.

The result is more often propaganda than history. Nevertheless, although many of his "historial" conclusions are absurd, his book is not wholly useless. He has read widely in the most fashionable scholarship, tries hard to be original (and too often succeeds), has a sharp eye for unnoticed details that can be made to serve his arguments, and writes in a clear and lively style. One learns while laughing.

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BARUCH HALPERN. *The First Historians: The Hebrew Bible and History*. San Francisco: Harper and Row. 1988. Pp. xviii, 285. \$22.95.

This book is about historiography in general and biblical historiography in particular. The biblical books Deuteronomy, Joshua, Samuel, and Kings are known to modern scholars as "the Deuteronomistic history." They give the story of the Israelites from the period of the wilderness wanderings until the destruction of the temple and the dissolution of the kingdom of Judah in 587/6 B.C.E. But the theological judgments, political leanings, and moralistic tone of the history are so pronounced that many modern scholars barely deign to regard it as a history at all. They trust neither the author, known as "the Deuteronomist," nor the information that is purveyed. In this book, Baruch Halpern argues that the Deuteronomist is a real historian and that the Deuteronomistic history is a real history.

In the opening chapter Halpern argues that history and romance are formally identical but substantively different. A history is an antiquarian work whose intent is to depict real events and persons of the past, but a novel has no such intent. Historians will have opinions and prejudices, to be sure, but they will avoid gratuitous decorations and free inventions. After a brief survey of modern scholarship on the Deuteronomistic history highlighting the contribution of Martin Noth, Halpern turns to the body of his demonstration. According to Halpern, the author of Joshua and Kings sought out reliable sources on which the narrative could be based. The Deuteronomist used these sources responsibly, supplementing them with imaginary "facts" only when necessary and reconciling them when they appeared to be contradictory. Even the later editors and redactors of the Deuteronomistic history were responsible historians who did not act wantonly with their sources and were dedicated as much to truthful history as to correct theology. These historians knew how to distinguish myth from history, metaphor from reality. Halpern's demonstration is effected through a series of detailed studies of individual chapters and sections in the Deuteronomistic history.

This book is in many ways an antidote to John van Seters's *In Search of History* (1983). Unlike van Seters, Halpern argues that the Deuteronomistic historian was engaged in fundamentally the same enterprise as Herodotus, Thucydides, Xenophon, and Polybius. Citations from and about these Greek historians stud Halpern's text. The Deuteronomist, no less than the Greek historians, reveals the authorial self-consciousness of a competent and serious historian. This is Halpern's fundamental point, but the point is assumed rather than proved. Unlike the Greek historians, the Deuteronomist did not write prefaces, did not reveal his name, and never stepped out from behind the narrative curtain to address the audience in the first person. The Greek historians have personalities; the Deuteronomistic historian has none, or at least none that is revealed explicitly. If we assume that the biblical author had an antiquarian's interest in the past, was determined to write a truthful account of previous events, and knew how to evaluate sources and distinguish fact from fiction, Halpern's demonstration succeeds. But the thesis is self-validating, and the initial assumption is questionable. And, even if we grant that the Deuteronomist intended to write history and attempted to do so accurately and fairly, we must still determine whether the Deuteronomist succeeded. Perhaps, in spite of his best efforts and his noblest intentions, the Deuteronomist wrote bad history. Authorial intent is surely one of the key separators between romance and history, but authorial competence is another. Halpern emphasizes that the Deuteronomistic historian intended to write good history, but Halpern does not devote enough attention to proving that he succeeded.

In sum, this book should interest all students of ancient historiography. Some of the chapters are heavy

going (see, for example, the first paragraph on page 138), but on the whole the book's style is engaging, sometimes brilliant. Halpern will convince those who want to be convinced.

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DOROTHY J. THOMPSON. *Memphis under the Ptolemies*. Princeton: Princeton University Press. 1988. Pp. xvii, 342. \$37.50.

Under the Pharaohs, Memphis was the principal city in Egypt; under the Ptolemies it was second only to Alexandria. Because of its location, the material remains of Memphis are better preserved than those of Alexandria. Dorothy J. Thompson uses mainly archaeological evidence and papyri along with occasional remarks in Greek literary texts to reconstruct the history of the city during the three centuries of Ptolemaic rule, adding a brief epilogue treating Roman Memphis. Five appendixes contain various lists that amplify and provide documentation for subjects treated in the text.

Whereas Alexandria was founded as a Greek city, Memphis retained much of its Egyptian character under the Ptolemies, despite the construction of a Serapeion. Thus, the subject is an odd choice for a Hellenistic historian who cannot read Demotic, particularly since the Egyptians were vigilant about recording their familial and business transactions, often in many copies. Under the circumstances, Thompson did the best she could. In the preface she acknowledges that she was aided by Demoticists and Egyptologists in working with non-Greek sources. Her footnotes indicate that, whereas she exploited primary sources in Greek, she often relied on secondary, synthetic works in using the Egyptian evidence. Moreover, the author distances herself from her subject by avoiding comments or speculations that would give readers a sense of the life of the community. For example, could there have been any connection between the ethnic and geographical endogamy Thompson traces among undertakers (namely, embalmers) and the physical realities of practicing such a profession in the heat of the Egyptian desert? Perhaps for these reasons, although the book contains interesting anecdotes, much of it lacks the vividness that infuses many contemporary works about preindustrial urban areas such as French villages and English parishes.

Thompson (writing under the surname "Crawford") had previously published valuable studies of several aspects of the Ptolemaic economy. In chapter 2 she describes an economy that must have existed in other parts of Egypt as well. Using documents from the archive of Zenon (named after a man who served as chief agent for a wealthy Greek official in the third century B.C.), Thompson discusses population, agriculture, manufacture, and trade. But the chief industry of

Memphis was death, specifically the mummification and burial of animals and human beings: "The greatest assets of the choachytai ['libation-pourers' in the cult of the dead] and undertakers remained the inhabitants of the area whom they viewed as prospective mummies" (p. 168). Chapter 5 treats the undertakers. For this industry the documents are Demotic. Thus, despite Thompson's attempts to give an integrated picture of life in Memphis, two economies emerge, one Greek and one Egyptian. Was this the reality, or is the distinction exaggerated by the schizophrenia of the sources?

Chapter 3 treats the migration and settlement of Jews and other Semitic groups not merely into Memphis but into Egypt, beginning with the arrival of Canaanites in the period of the New Kingdom. Many immigrants were traders or mercenaries. Thompson ignores the possibility of mixed marriages, however, as a factor accelerating the assimilation of minorities.

The remaining three chapters deal with cults at Memphis and constitute approximately one-third of the volume. In Egyptian religion, divinities were often associated with animals through which their spirits were manifested. In the opening sentence of chapter 7, Thompson misrepresents the belief system by stating that "the aspect of Egyptian religion which above all startled and intrigued outsiders, then as now was the worship accorded to animals" (p. 190). Rather, the spirit of a god could be manifested through a living animal. The Egyptian priests distinguished between divinities and the animals associated with them. For example, one bull at a time was used for the cult of Apis at Memphis. When each bull died it was mummified and buried, but it was believed to be resurrected like Osiris. A feature that I find more startling than theriomorphism was the popularity of the cult of Serapis, a divinity patently manufactured by the Greeks in the Ptolemaic period (although perhaps incorporating pre-existing Egyptian elements) and foisted on the ancient cult of Isis and her brother-consort Osiris.

As so often happens in ancient history, it is necessary to generalize from a limited number of examples. In this case the source was the Serapeion archive, a collection of documents of one man, his family, and associates. The archive indicates that the syncretism of the cult of Serapis at Memphis is a symptom of the mixed, bilingual society that evolved especially among upwardly mobile inhabitants of what had been a purely Egyptian sacred city.

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MICHAEL WHITBY. *The Emperor Maurice and His Historian: Theophylact Simocatta on Persian and Balkan Warfare*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1988. Pp. xiv, 388. \$72.00.

The reader of Theophylact Simocatta's *Universal History*, the last extant representative of late Roman historical craft, confronts a paradox. For, although the ties of literary and generic continuity are clear, the *Universal History* also evidences the shifting literary and thematic patterns that announce Byzantine literature's separation from late Roman. In addition, Theophylact is one of our best sources for the Eastern Mediterranean and Balkans for the last years of Roman hegemony before the Persians, Arabs, and Avars swept away New Rome's power. Theophylact is thus pivotal for both the evolution of the classical tradition and the political, social, and military conditions of the late sixth century. He is now the subject of Michael Whitby's study, which follows Whitby's elegant translation with Mary Whitby of the *Universal History*.

The book is meticulously researched and highly readable, and, because the methodological foundation is *Quellenkritik*, the reader appreciates Whitby's fine organization of his material. The method is necessary because Theophylact wrote nearly fifty years after Maurice's accession, and Whitby must identify Theophylact's sources, mostly lost, in order to evaluate his accuracy. As Whitby reveals Theophylact's sources, he uncovers the temporal ballast necessary for his reconstruction of the chronology and geography of Maurice's wars against Avars and Persians. Additionally, he uses the *Strategicon*, the late sixth- or early seventh-century military manual, to elucidate the strategies and, when possible, the tactics of Maurice's army. Finally, Whitby discusses the stylistic anomalies of the *Universal History* and, to a certain extent, the cultural context of Theophylact, although here Whitby largely follows Averil Cameron's interpretation of late antique literary and cultural evolution.

Whitby's *Quellenkritik* is necessary for the distillation of a text that is at times opaque, but his method rests on an implicit assumption that Theophylact preserved his sources with few, if any, editorial changes: the less Theophylact manipulated his sources, the greater his reliability. Thus, for Whitby, Theophylact was a "passive historian" (p. 311), with "an absence of personal interpretation" (p. 323). Whitby certainly recognizes the danger inherent in assigning biases to Theophylact's sources that might have been Theophylact's own (see p. 94), but there is no discussion of what might have been Theophylact's biases or how his biases might have led him to rework his sources, for rework his sources Theophylact certainly did.

Justinian the general's description of Christian martyrdom in battle in book 3, for example, characterizes the 620s, when Theophylact wrote his work, rather than the 580s and 590s, and Whitby himself concedes that Theophylact must have reworked (if not invented) the speeches and at least some of the narrative surrounding the Persian king Chosroes in the light of Chosroes's attacks on the empire after the reign of Maurice. More generally, Whitby never comes to grips with the question of why Theophylact should have written of earlier events (he did not arrive in Constanti-

nople until Maurice had been long dead). Whitby, who never explores the implications of Theophylact's imperial patronage, simply dismisses Herbert Hunger's suggestion that Theophylact wrote to rehabilitate Maurice and blacken Phocas (Maurice's executioner, Heraclius's victim, and Theophylact's *bête noire*) in order to legitimate Theophylact's patron, the usurper Heraclius (p. 350, n. 8).

One finds no hesitation on the part of Theophylact's contemporaries to rewrite the past in the interest of their imperial patron. The author of the *Paschal Chronicle* and the court poet George of Pisidia, who were the literary clients of the same patrons as Theophylact, saddled Phocas with the loss of the Balkans to the Avars and the eastern provinces to the Persians when, in fact, these areas were largely lost by Heraclius. In addition, they failed to mention the protracted and bloody fighting in Egypt and Asia Minor that took place during Heraclius's revolt against Phocas, and it is at least worth consideration whether Theophylact did not pursue the same sort of suppression of facts and insertion of misleading information.

Undoubtedly, Whitby's book lights a way for future scholars to study the last decades of the classical world. His generally solid source and chronological groundwork will form the foundation of any discussion of the literary evidence for Maurice's reign. Whitby's goal, to use Theophylact to analyze the reign of Maurice, does not, admittedly, require an extended discussion of the political culture of Theophylact's time. Without it, however, the reader might entertain doubts about Theophylact's "passive" use of sources and Whitby's literal use of the *Universal History* as the unchanged duplication of earlier texts. In a sense, the book is misnamed, for, although Theophylact wrote about Maurice, his emperor was, in fact, Heraclius, and the temporal and political gap between historian and subject cannot easily be bridged by a chain of sources.

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CHESTER G. STARR. *The Influence of Sea Power on Ancient History*. New York: Oxford University Press. 1989. Pp. 105. \$16.95.

"How different things might have been could Hannibal have invaded Italy by sea," wrote Alfred Mahan. Musings such as this resulted in his celebrated book on the paramount role played by sea power. Chester G. Starr, well known for his numerous studies of the ancient world and particularly its naval aspects, argues in this succinct volume that, contrary to a widespread belief, "instead of viewing sea power as an important element in the course of ancient history, we must expect it to be a spasmodic factor, though at points it does indeed become a critical force" (pp. 5-6).

Starr reviews the highlights of ancient maritime history: the early development of communication by water; Carthage's domination of the western Mediter-

anean; Athens's career as a naval power par excellence; the emergence of the massive Hellenistic naval aggregations; the acquisition by Rome, a land power, of a fleet that ended Carthage's domination and eliminated the ponderous Hellenistic fleets; the spread of piracy when Rome disbanded its navy; the creation of a new Roman fleet that eliminated the pirate menace, served in the civil wars, and formed the core of the imperial navy.

Starr presents Athens of the second half of the fifth century B.C. as the best example of what Mahan had in mind: its mastery of the Aegean guaranteed absolute control over the economy and politics of the area. Yet, he emphasizes, this situation was short-lived and was ended not by a stronger seapower but by a land power that temporarily acquired a fleet. Carthage is the next best example: it ruled the western waters for centuries. Yet it, too, was overthrown by a land power that temporarily acquired a fleet. The history of these two states reveals undeniably the importance of sea power in ancient times, but, Starr argues, we must bear in mind that both were overcome by "opponents [whose] strengths of organization and determination as perfected on land were decisive in their success [on the sea]" (p. 84). I am not sure that this fact demonstrates, as Starr seems to think, how wrong it is to apply Mahan's views to antiquity. Starr holds that Mahan was mistaken about Hannibal himself; Hannibal, Starr claims (p. 58), even had he enjoyed control of the sea, could not have transported his forces by ship because of his large numbers of cavalry and elephants. But the Ptolemies had long been shipping elephants hundreds of miles up the Red Sea, and, if Carthage's navy had still been master of the western Mediterranean, it could well have supplied the two or three hundred horse-transporters his cavalry would have required.

The narrative is fast moving, lucid, and documented by references to the major studies. It is aimed at the general reader but will mean most to those who are familiar with at least the general outlines of Greek and Roman history.

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CYNTHIA FARRAR. *The Origins of Democratic Thinking: The Invention of Politics in Classical Athens*. New York: Cambridge University Press. 1988. Pp. x, 301. \$37.50.

This is an important book. It builds on the work of communitarian political theorists to advance the debate on the nature and significance of democracy, ancient and modern. It also challenges the widespread view that the early Greeks failed to develop a theory of democracy. Cynthia Farrar finds precisely that, not in Plato and Aristotle, whom she sees as encouraging a "retreat from politics" (p. 265), but in three fifth-century thinkers—the sophist Protagoras, the scientist Democritus, and the historian Thucydides. Because their thinking about democracy was phrased in con-

crete rather than in abstract terms, modern scholars have failed to recognize their significance. Each of the thinkers explored the question of how a polis might simultaneously secure the autonomy of the individual and the common good. They all rejected the idea of a transcendent order and shared the apprehension of imminent social dissolution, but they also experienced a democracy that, from time to time at least, was led by reason and guided by communally minded leaders.

Of the three thinkers discussed in detail, Protagoras should be recognized as the "first democratic theorist in the history of the world" (p. 77). Democritus, too, had a clear vision of the relationship between the individual and the social order, but the ties he envisioned bound individuals as human beings rather than as citizens of a specific community. His thought is thus universalist rather than strictly political and, hence, in Farrar's view, tends toward social atomism.

In Thucydides, however, she finds a thinker who recognized that in human energy, intelligence, imagination, and the capacity for reflection lie both the danger—the drive to domination and exploitation—and the solution to the problems of civic life, provided that individuals shape their understanding of themselves through a vigorous political process, where participation is broad and where leaders of judgment and restraint are in command.

Is this anything more than an elegant reformulation of the old clichés that presented Athenian democracy as an illustration of the need for enlightened leadership and moderation? The originality of Farrar's view lies in her insistence that Athenian politics was not simply an area for competitive display and the advancement of individual interests but a way in which individuals could understand themselves and reflect on their situation. It could thus reconcile individual interests and communal needs, tolerate intense disagreement and controversy, and encourage reflection about personal and civic concerns.

Farrar goes on to make a stronger claim. Not only did these early Greeks develop a theory of democratic politics, but their thought and the historical experience of the period are also of special significance to a time like ours when citizens are in danger of losing confidence in politics. Ancient Athens provides a "case-study of the possibility of combining social integration with active political participation and sharp political controversy" (p. 276). It encourages reflection about political life and recognizes that our self-understanding can be shaped through participation in politics.

Much of Farrar's case depends on her thoughtful exegeses of the ancient texts. Her treatment of each of the major thinkers that she examines might be faulted—the view of Protagoras as a rigorous thinker responding to problems posed by the Eleatic school is hard to reconcile with Plato's critique of his muddle-headedness. Protagoras might be more plausibly presented as responding less to philosophical problems than to the social issues of his time, including the aristocrats' claims to power. Her presentation of Dem-

ocritus might also be challenged on technical grounds, as excessively dependent on maxims attested only in late sources. And Farrar's Thucydides is rather more optimistic than most critics today would allow. Yet her analysis is powerful enough to carry the argument over such obstacles and to bring these texts into dialogue with questions that affect any society that sees itself confronted with social fragmentation. The result is a powerful book on a topic of major significance.

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ELLEN MEIKSINS WOOD. *Peasant-Citizen and Slave: The Foundations of Athenian Democracy*. New York: Verso. 1988. Pp. x, 210. \$40.00.

Athens in the fifth and fourth centuries B.C. is the first democracy about which we are well informed and very likely the first anywhere for a complex and populous society (some thirty thousand adult male citizens). Its political functioning is well understood: males born of citizen parents had the right to vote, speak in assembly, hold office, and receive equal treatment from the law. But Athenian social and economic structures are matters of debate. There were many chattel slaves (at least half as many as there were citizen men, women, and children) and many resident foreigners. Athens had a relatively large territory for agricultural exploitation and probably the largest concentration of workshops and the greatest number of consumers of any state on the Aegean Sea. For over seventy years it was also the domineering leader of some two hundred smaller political entities that constituted the Athenian "empire," and its private and public holdings overseas were unmatched. The question is how these various factors related to the political, ideological, and cultural systems, a problem complicated by the changing importance of each factor in the course of the two centuries.

The "central thesis" of Ellen Meiksins Wood's book is that "the distinctive character of Athenian democracy was . . . the extent to which it *excluded* dependence from the sphere of production" (p. 82), that is, that production, primarily agricultural, was the result not of the labor of socially, politically, or juridically dependent classes, as in the ancient Near East and much of medieval Europe, but of what Wood calls "peasants" or "small-holders," who were full members of the society. From the decision made by Athens, under the guidance of Solon early in the sixth century B.C., to abolish dependency and to guarantee the freehold of farmers, Wood derives the democratic and political system and the cultural and intellectual inventiveness of classical Athens. It is essential to Wood's argument that democracy thereafter was not based on or even much affected by the exceptionally high proportion of chattel slaves to citizens and, in particular, that slaves played no large part in agriculture, which she takes to be the basis of the economy. Those, myself included, who see the role of slaves in agriculture as a supplement to the labor of

most landowners themselves are severely taken to task. Wood attempts to offer a rationale for what one might think, on the face of it, an odd exclusion in the light of the widespread use of slaves in nonagricultural activities. Here it is impossible to argue details, but, on the importance of slavery in agriculture, I remain quite unrepentant, without being wedded to the particular model offered to explain how and when slaves came to be used. Readers interested in Greek slavery would do well to consult Yvon Garlan's *Slavery in Ancient Greece* (1988), which has been available in French since 1982 but is not used by Wood.

As one who shares Wood's enthusiasm for Athenian political and social achievement but sees it as considerably more problematic and contradictory, I would stress that calling Athenian society one of "peasants" begs the question. How many farmers were "small"? Athens usually had no less than ten thousand heavily armed infantrymen (hoplites), most of whom had at least one attendant (see W. K. Pritchett, *The Greek State at War*, vol. 1 [1971], pp. 49–51) and who would have occupied most of the agricultural land (see Chester G. Starr, *The Economic and Social Growth of Early Greece* [1977], p. 155). Many historians would see the decisive shift from a hoplite-oriented society to one that at last offered the poorest citizens a real place in political life as coming with the Ephialtic reforms of circa 460 B.C., while some scholars are doubtful about the degree of their actual participation after the loss of the "empire" at the end of the fifth century. In any case, it is far from clear that most or many of the poorer Athenians were subsistence "peasants." Looking beyond Athens one should note that most Greek city-states did not have dependent labor, yet few came close to matching Athens in democracy.

The book is argumentative, even polemical in style. It does not set out how Athens worked and therefore is not helpful to those who may have become interested in Athens for the first time. But it is a vigorous if prolix contribution to a worthwhile debate, and it includes incidentally interesting exposés of the "myth of the idle mob" from the eighteenth to twentieth centuries and of the facile assumption of technical stagnation stemming from slaveholding.

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DONALD KAGAN. *The Fall of the Athenian Empire*. Ithaca: Cornell University Press. 1987. Pp. xviii, 344. \$39.50.

With this volume, Donald Kagan has brought to completion his masterful study of Athenian imperialism and the Peloponnesian War. The previous volumes (also published by Cornell University Press), *The Outbreak of the Peloponnesian War* (1969), *The Archidamian War* (1974), and *The Peace of Nicias and the Sicilian Expedition* (1981), carry the story from the origins of the Delian League and its transformation into the Athenian empire, through the great struggle between Ath-

ens and Sparta at the head of the Peloponnesian League, to the Athenian disaster in Sicily in 413. This book covers the final years of the war, from 413 to 404, with the same attention to political analysis and military detail as its predecessors. But it is different in one important respect. Previously, Thucydides was the primary source for both Greek history and Kagan's analysis, but, when the narrative abruptly breaks off at the end of Thucydides' unfinished eighth book, it is necessary to rely on the much inferior history of Xenophon and to give even more prominence to other, later sources, such as Plutarch and Diodorus, which were used to supplement Thucydides in earlier parts of the story.

Kagan's interest in Thucydides is revealed by the fact that more than half of the book is devoted to only three years, 413 to 411/10, with six chapters concerned primarily with the events surrounding the revolution that briefly replaced the Athenian democracy with oligarchy in 411. Here Kagan provides detailed analysis of the political, diplomatic, and military events relevant to the revolution, but sometimes his suggestions about the motivations of the principals, such as Peisander, Phrynichus, and Alcibiades, are rather speculative and will not convince every reader. The later chapters illuminate the importance of the Spartan-Persian accord in the ultimate defeat of Athens, as well as the defects of the restored democracy, whose limitations are vividly revealed, for example, in the chapter on the trial of the generals following the battle of Arginusae. Cyrus and the Spartan admiral Lysander take their place alongside the Athenian leaders Alcibiades, Theramenes, and others in the final dramatic years of the war.

The book is solidly grounded in the ancient sources, which Kagan knows very well, and in the modern literature; indeed, a number of the books and articles cited in the notes and bibliography were written by Kagan's former students. This work is essential for anyone who seeks to understand the nature of politics and diplomacy in classical Greece and the outcome of Athens's conflict with Sparta. It also represents a significant contribution to ancient historiography, particularly to an understanding of the works of Thucydides. Although one needs some background in Greek history to appreciate to the full the book's arguments, it is written clearly, and it flows well. Thus, one need not be a specialist to profit from Kagan's analysis. With its three predecessors, this volume will long stand as the definitive work on the Peloponnesian War and the nature of the Athenian empire; it will also rank among important studies of Thucydides, one of the most challenging and demanding writers of classical Greece. In his final chapter, titled "Conclusions," Kagan recapitulates the course of the war, pausing often to note various ironies at junctures of the conflict. His final observation gives us pause to reflect: "The Athenian experience in the Peloponnesian War suggests that in warfare democracies, where everything must be debated in the open and relatively uninformed majorities

persuaded, may find it harder to adjust to the necessities of war than other, less open societies. Perhaps that is what Thucydides had in mind when he connected the Athenian defeat with the death of Pericles" (p. 426). If Thucydides was correct in his assertion that the future resembles, if it does not reflect, the past, there may well be a lesson here for us today.

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MICHAEL H. CRAWFORD. *Coinage and Money under the Roman Republic: Italy and the Mediterranean Economy*. (Library of Numismatics.) Berkeley and Los Angeles: University of California Press. 1985. Pp. xxv, 355.

This book is not so much a study of Roman coinage of the Republic, although that is briefly reviewed, as it is, first, a survey of the non-Roman coinages in areas that Rome came to dominate and, second, a sketch of the changes in, or disappearance of, those coinages as the Roman coinage displaced them. Historians will be interested in the efforts made to relate the various coinages (and coin finds, sometimes even stray coins) to economic and political history. There are sixty appendixes of finds, that is, the raw materials used as evidence (some useful, some not), plus eighteen other appendixes. Chronologically, the book deals with the period from the sixth century B.C. to the reign of Augustus. Geographically, it embraces all of the regions that Rome conquered.

Michael H. Crawford begins with Italy; he surveys the peoples of the peninsula and then discusses the rise of coinage among the Greeks and Etruscans there and finally at Rome itself. Other areas are presented in a similar pattern. There are numerous illustrations of uneven quality, frequently of coins rarely illustrated. The coins discussed are often seen as struck for the use of Rome in its expansion. The author attempts a kind of integration not done before: for example, the early Roman coins minted in south Italy are also seen as a stage of Campanian coinage.

Roman monetary penetration everywhere caused other coinages gradually to disappear or conform to the Roman model; local, mostly bronze coinages continued in the East, even into the period of the empire. These, too, are briefly surveyed. Crawford is convinced, contrary to some evidence, that they required no authorization from Rome.

Those who expect the book to dovetail with Crawford's earlier monumental work, *Roman Republic Coinage* (1974), will be disappointed. But it is necessary to have that study at hand when reading this volume, for some coins are mentioned only by number in *Roman Republic Coinage*, and there are frequent cross references. *Coinage and Money* does supplement the earlier work and sometimes modifies or repeats it. Commendably, Crawford has dropped his odd idea that the early Roman coinage was controlled by the censors. Unfortunately, he still holds that Rome issued no silver

coinage for a quarter century or so before 157 B.C. (Crawford is right that hoards are the best evidence for relative dating, but, when there are few or incomplete hoards, his dating suffers.) Without solid evidence, he argues that, for a long period before Sulla, Rome paid for most expenditures in new coin only. (Did the government really melt down perfectly good denarii received in payments to the treasury?)

Crawford can suggest something as a possibility and later refer back to it as fact (pp. 174 and 187). And he does the same thing indirectly: in *Roman Republic Coinage*, Crawford presented estimates of coin volume based on what might fairly be termed guesstimates of the numbers of dies used. Keith Hopkins rather naively accepted Crawford's estimates as fact (in "Taxes and Trade in the Roman Empire [200 B.C.—A.D. 400]," *Journal of Roman Studies* [1980], page 106, Hopkins writes, "His data tell us the amount of silver coins issued in Rome each year in the period 157–50 B.C."), and he produced a graph based on that "data." Crawford reproduces the graph here with no indication that his own guesses lie behind it. One must admit that, if we had actual mint records, the graph might indeed look something like the one Crawford presents. But even wary historians need to know that they should exercise the greatest caution when using the work of numismatists.

Crawford can be cavalier with the work of other scholars, dismissing some brusquely and making no mention of important works that he certainly knows. The oversupply of appendixes and even maps (five, all of the same area, are used in one eight-page chapter) plus some illustrations that seem not to relate to the text have made the book unnecessarily expensive. Still, this is a good and useful book and it should be in every research library.

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DAVID JOHNSTON. *The Roman Law of Trusts*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xiii, 306. \$64.00.

Caveat lector. This challenging case study in legal evolution offers a fresh approach to a neglected, yet important, topic. At the same time the book is not as wide in scope as the title may suggest. David Johnston claims to have written this work not just for Roman lawyers but also for "other legal historians and (perhaps less so) Roman historians" (p. viii). In view of those parentheses, a reviewer who is a historian and Johnston's intended audience may both be forgiven for approaching the volume apprehensively. And, to be sure, despite all of the author's efforts, readers cannot help but find much to deter them. The principal extracts from the jurists are translated, but the text still demands a grasp of Latin (try pages 210–11 without it), and, inevitably, the background to the battery of legal

matters touched on cannot be filled out. English law, moreover, is taken as the principal early modern counterpart for comparison.

But, for all of the obstacles, historians ought to persevere. Not only will they find the presentation unfailingly lucid, but they will also value Johnston's consistent practice of summarizing his conclusions, usually, though not always (note pp. 186–87, 256–57), at the end of the sections concerned. They will discover in him a rare ideal, namely an expert in Roman law who holds “that the history of the institutions of Roman law is an important, even a vital, ingredient in any attempt to write Roman social history” (p. viii). In consequence, practicalities, the efficacy of the rules, and the economic and social impact of legal changes in trust law are all issues of significance to him. At the same time he sheds much light for the social historian on uses of secrecy (chap. 3) and on intestacy (pp. 151–54). Of even broader interest for historians is his discussion (pp. 272–79) of why actionable trusts were introduced during Augustus's reign. This discussion may be related to wider controversies about the purpose of much of that emperor's social legislation and in turn has an impact on two other related questions: first, the contrast between *utilitas* and *elegantia*, that is, “how far the course of development of any legal institution is conditioned by external social factors, and how far it is a matter of the ‘autonomous’ development of legal doctrine in the hands of lawyers” (p. 271); and, second, the question of the emperors' personal contribution to the making of law. The first query is raised by Johnston, but the second is not. He simply rates Augustus's contribution highly and chooses not to pursue the question for any of his successors.

That is understandable when such a quest might move too far from the book's central theme, that is, the remarkably dynamic evolution over the six centuries from Cicero to Justinian of the Roman law of “trusts” (*fideicommissa*) as it applied specifically to property. Johnston does not aim to address trusts of freedom to slaves, much less to furnish a textbook on *fideicommissa* or a comprehensive social history of them—all tasks adequately taken in hand by others (p. 8). Rather, after setting out the background and the Augustan legislation (chaps. 1, 2), it is his purpose to modify received opinion that the two special merits of trusts were to benefit individuals disqualified at law and to facilitate the establishment of perpetual settlements (chaps. 2–4). He then discusses the role of trusts in intestate succession, with particular attention to their astonishing breach of the distinction between testate and intestate succession (chap. 5). Matters of interpretation and procedure are covered in chapters 6 and 7; chapter 6 is the most technical part of the book. Finally, chapter 8 focuses on Justinian's eventual fusion of legacies and trusts—*per omnia exaequata sunt legata fideicommissis*—and indicates some grounds for reassessment of the degree of Roman influence on this area of English law.

Since Johnston deliberately limits himself to consideration of certain types of *fideicommissum*, some fuller

reassurance that his general conclusions can justifiably rest on these alone would have been welcome. There is no question, however, that his book has succeeded splendidly in its principal aim of rescuing the law of trusts from longstanding neglect: in fact, it has placed the subject in a whole new light.

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MEDIEVAL

J. H. BURNS, editor. *The Cambridge History of Medieval Political Thought, c. 350–c. 1450*. New York: Cambridge University Press. 1988. Pp. viii, 808. \$89.50.

This important book, a collaborative effort, aims at presenting “a conspectus, as comprehensive as possible within prescribed limits of space, of the present state of historical scholarship in the field surveyed” (p. 7). It is to be followed within a year by a successor volume on the early modern period, and Cambridge University Press announces it as the first in a series that “will eventually take the history of political thought up to the twentieth century.” The work remains within the uneasy chronological boundaries traditionally assigned to the Middle Ages (though not without some editorial agonizing), and it pays the “great thinkers” their proper due. The authors' concerns, however, do not revolve around the contributions of those thinkers alone. Correctly concluding that a “thematic or conceptual approach” is likely to be “more fruitful in the history of medieval political thought” (p. 4), John H. Burns organized the work into four chronological sections, each introduced by chapters on the general historical background to the period. The first section is foundational and includes chapters by Henry Chadwick on Christian doctrine, by John Procopé on Greek and Roman political theory, and P. G. Stein on Roman law. (To this section is appended a subsection contributed by D. M. Nichol on the history of Byzantine political thought.) The second section focuses on the period of “beginnings” stretching from circa 350 to circa 750, with chapters by R. A. Markus on the Latin fathers and P. D. King on the barbarian kingdoms. The third is entitled “Formation: c. 750–c. 1150” and includes chapters by R. van Caenegem on government, law, and society; Janet Nelson on kingship and empire; I. S. Robinson on church and papacy; and D. E. Luscombe and G. R. Evans on the twelfth-century renaissance. The final, and most unwieldy, section, “Development: c. 1150–c. 1450,” includes chapters on spiritual and temporal powers (J. A. Watt), law (K. A. Pennington covers the period from 1150 to 1300; J. P. Canning, from 1300 to 1450), government (Jean Dunbabin), community (Jean Quillet discusses community, counsel, and representation; Antony Black, the conciliar movement), the individual and society (Antony Black), and property and poverty (Janet Coleman). Burns provides a brief introductory essay and a

conclusion; there are thirty-four extremely helpful pages of biographical notes on the pertinent medieval authors; and a seventy-four page bibliography is appended.

The volume betrays the strengths and weaknesses one has come to expect of the collective or team approach characteristic of the Cambridge histories, although the team, this time, is not really an international one but drawn almost exclusively from scholars working at universities in the British Isles. The strengths of the volume are the authors' familiarity with the subfields and the control of the pertinent scholarly literature that one can expect experts to bring to their task, a variety of scholarly viewpoints that stimulates more often than it confuses, and (in this instance, at least) the advantage of having the leading thinkers of the period interpreted for the reader by more than one contributor and from more than one angle. The weaknesses are an occasionally irritating amount of overlap and repetition and the understandable absence of the single architectonic vision that makes the great contributions of Otto von Guericke, A. J. Carlyle, and Walter Ullmann to the history of medieval thought so very stimulating, and no less so when one does not agree with them. In the case of this particular venture, I would judge the strengths to be more in evidence than the weaknesses. It is not easy to impose a firm sense of order on the complex strands of thought and lines of institutional development pertinent to political thinking in the high and later Middle Ages, and the last section of the book does indeed lack that organizational clarity. In this it is not helped by Quillet's lengthy and rather rambling contribution, and Coleman's chapter on property and poverty, valuable and interesting in itself, is poorly integrated with its neighboring chapters. But the overall level of the scholarship is very good indeed, and the book is punctuated by a whole series of excellent chapters. Those by Markus, Nelson, Pennington, Watt, Canning, and Black, all of them at once both lucid and meaty, strike me as especially valuable. Burns and the Cambridge University Press are to be congratulated on producing a truly substantial publication that every historian with a serious interest in medieval political thought will find, henceforth, indispensable.

This having been said, it remains to indicate seriatim some of the thoughts to which this survey, accurately billed as comprehensive and authoritative, gives rise. First, and it is perhaps a comment on the way in which medievalists tend to be trained, I note the fact that the contributors usually succeed better in integrating the history of political thought with political and social history than with the broad movement of medieval philosophy and theology. Second, a surprising paucity of scholarly attention has been given over the years to the intriguing anonymous commentator on the Pauline epistles who has generally been known since the time of Erasmus as Ambrosiaster and who was notable among fourth-century authors for the extremely lofty hierarchical position he ascribed to the ruler, the *imago dei*.

Third, the sheer originality of Augustine's political vision is striking but easily concealed from us by the prevalence during the Middle Ages of an "augustinisme politique" that represented, in fact, a distortion of his views. In the medieval era, his position was never quite grasped in its full complexity; to his contemporaries, so many of them Eusebian accommodationists, it was something of a novelty. Fourth, the issue of sacral kingship is handled in a dismissive manner. Despite all that the anthropologists and students of comparative religion have told us about its ubiquity world wide and across so great a stretch of time, King's field of vision appears blocked by his preoccupation with routing old nineteenth-century assumptions about the existence of a homogeneous *Germanentum*. As a result, when it comes to the matter of the presence among the continental Germans in late antiquity of the institution of sacral kingship, his approach is robustly provincial. However widespread the institution elsewhere (a fact he does not acknowledge), the burden of proof is placed squarely on the shoulders of those who would assert its presence in Europe and not, as one might have expected, on those who reject that possibility. Fifth, in this volume, Walter Ullmann's influence is markedly muted. In terms of overarching interpretations of medieval political thinking, Guericke and Carlyle get just as much play, and Ullmann's misleadingly beckoning invocation of a distinction between "ascending" and "descending" conceptions of government is appropriately handled in gingerly fashion. His voice, however, is not entirely silent. One catches echoes of it in Canning's willingness to speak, without any apparent sense of the danger of anachronism in so doing, about Aristotle having provided thirteenth-century Europeans with "a ready-made theory of politics and the state as existing within a purely natural and this-worldly dimension" (p. 360). Happier echoes, it should be added, can also be heard: in the emphasis the contributors place on the importance of the Vulgate's role in transmitting Roman political concepts to medieval thinkers and in the prominence given to the canon law as a profoundly formative influence on the development of medieval political thinking. Sixth, in considering the sharp contrast between Christian and Islamic political thinking during the Middle Ages, it is well to be reminded that, whereas by the tenth century Plato's *Timaeus*, *Republic*, and *Laws* had all been translated into Arabic in their entirety (the last two were not translated into Latin until the fifteenth century), Aristotle's *Politics*, so crucial an inspiration in Latin Christendom from the thirteenth century on, may never have been translated into Arabic at all. Seventh, Pennington asserts that "the secular-mendicant controversy [which broke out in the 1250s] occupies a central place in the development of western political thought" (p. 453). He advances this claim with great force, arguing that, "by shifting the discussion from 'rights' of subjects to the origins of jurisdiction and political authority, mendicant theologians cleared the way for an irresistible emphasis upon the all-encompassing and pervasive

authority of the prince" (p. 453). It is to be hoped that this view will now find its place in our general histories of Western political thought. Finally, the concluding chapters of the volume remind one how difficult it is to be sure that one really understands what was involved in late medieval appeals to popular consent. Rightly stressing the extreme elasticity of the medieval concept, Dunbabin tentatively concludes that, "while many intellectuals believed popular consent strengthened government, only a minority thought it fundamental in that it conferred legitimacy" (p. 518). The judgment seems reasonable. But the whole issue of the development of doctrines of consent in European political thinking is dauntingly complex, and it is not one open to resolution within the chronological confines of the present volume. For the prospect of such a resolution, we must now look to the next volume in the series, the publication of which is scheduled for 1991.

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ROBERT FOSSIER. *Peasant Life in the Medieval West*. Translated by JULIET VALE. New York: Basil Blackwell. 1988. Pp. 215. \$39.95.

From the tenth to the fourteenth centuries, Robert Fossier argues, peasant life in Western Europe acquired the peculiar characteristics that shaped rural culture well into the modern age. As in his earlier works, he insists that the Carolingian age saw no lasting changes or improvements in agriculture. His emphasis on this point, though controversial, highlights the importance of his chosen period in economic history. This book is not, however, primarily an economic study. Fossier attempts a synthesis of recent research on the nature and reality of peasant existence in order to stimulate interest in still-neglected or imperfectly researched areas. In combining insights from anthropological and archaeological perspectives, his book is largely a success.

Fossier opens with a description of peasant population growth, beginning in the tenth century and culminating in the mid-thirteenth century. His survey offers little to surprise the reader, although some sections stand out for their clarity and well-chosen examples. The chapter on the growth of the village system and on the consequences of inheritance practices is particularly effective. He also has much to say about changes in agricultural techniques, efforts by peasants to improve yields or produce for the growing urban markets.

In the later thirteenth century, population growth slackened as rural conditions deteriorated for many peasants. Fossier does not reject the notion that climatological conditions may have worsened, but he believes that the demands of urban markets were the main cause of declining living standards. Production of crops for city markets brought wealth to some peasants but poverty and scarcity for many more. Nobles were also

affected; many sold their lands for quick cash returns. Fossier's description of noble impoverishment is quite sophisticated, employing numerous examples to illustrate local variations. The growing influence of cities and of urban elites was apparent by the early 1300s.

Throughout the book, Fossier rejects the vision of rural continuity, emphasizing instead the many changes and developments affecting peasants. The static world of the peasant is a myth that Fossier delights in overturning. He also incorporates much information on the world of the peasant imagination—beliefs, customs, and folklore.

Naturally, there are problems with any work of synthesis, as Fossier clearly realizes. He states at the outset that "one of our prime aims must be at least a relatively consistent treatment across the period and covering the whole geographical area [of Western Europe]" (p. 3). But the select bibliography, topically organized, gives the impression that outside of France, Fossier's research has been rather spotty. For example, in a section titled "Society and the Economy in Different Countries" (p. 197), Italy receives only five entries, none later than 1964. Not surprisingly, the survey of Italian agrarian developments is sketchily treated in the text. Other problems arise from generalizations, unavoidable perhaps in a work of this nature.

But, for the scholar, the major problem with the book is the absence of bibliographical references. Fossier occasionally refers to an author or a source in his text but provides no notes. The bibliography does not, according to the translator, aim to be comprehensive, but it should at least include authors and works mentioned in the text. The lack of references will thwart the reader who wants to pursue further research. As a result, the book is stimulating to read but ultimately frustrating for the student of medieval history.

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ROBERT CHAZAN. *Daggers of Faith: Thirteenth-Century Christian Missionizing and Jewish Response*. Berkeley and Los Angeles: University of California Press. 1989. Pp. vii, 226. \$39.95.

Since the middle of the nineteenth century, historians of the Jews have sought to determine the point in the Middle Ages at which a physically secure, economically prosperous, and culturally vibrant medieval European Jewry began its well-known course of decline. For nearly as long, investigators have also considered the various contributions of the Catholic church to this historical process: anti-Jewish legislation, inquisitorial proceedings, efforts at proselytization, and a theology that spawned a popular image of a satanic Jew, perpetrator of the most heinous crimes against society and humanity. And, during the past two decades, the particular importance of the twelfth and thirteenth centuries in the evolution of Christian anti-Judaism has

repeatedly drawn the attention of writers and researchers.

Robert Chazan's book adds to the recent scholarly contributions of Bernhard Blumenkranz, Judah Rosenthal, David Berger, Chen-Melekh Merchavya, Kenneth Stow, Chazan himself, and others with a survey of Christian missionary activity among European Jews of the thirteenth century. Through an analysis of contemporary documents of religious polemic, Chazan seeks to illuminate the phenomenon of "mid-thirteenth-century missionizing as such." These efforts, he submits, shed light on "the climate of mid-thirteenth-century western Christendom, a period during which vigorous self-confidence combined with uncertainty and insecurity moved the leadership of western Christendom to press for enhanced homogeneity within and for expansion of the borders of Christian domination without." Finally, this history of Christian missionizing, he argues, reveals late medieval European Jewish communities as still vital, though increasingly oppressed, and still sufficiently "capable of analyzing the innovative arguments of their foes, identifying weaknesses in these new claims, and mounting wide-ranging counterarguments" (pp. 2-4).

Chapter 1 of this book identifies three characteristics of "serious" Christian missionizing: an ecclesiastical willingness to allocate substantial resources to such efforts, the development of special techniques for demonstrating the truth of Christianity, and the elaboration of convincing arguments sensitive to the ideas and milieu of the targeted audience. Before the thirteenth century, Chazan concludes, Christian efforts to proselytize among the Jews were too sporadic and pursued too haphazardly to amount to a serious challenge. Chapter 2 discerns precisely these three characteristics in church policies of the thirteenth century and attempts to explain why they then emerged. Drawing extensively on *Milhemet Mizvah* (*An Obligatory War*), a Hebrew compendium of anti-Christian polemic from mid-thirteenth-century southern France, Chazan considers in chapter 3 ("Coercion in the Service of Christian Truth") the conversionist sermon and public disputation in which Jews were compelled to participate, two techniques that typified the new policy of proselytization. Chapter 4 dwells on the intensification of older conversionist arguments as evidenced in *Milhemet Mizvah*; chapters 5 and 7, concerning Dominican friars Paul Christian and Raymond Martin, respectively, describe a new manner of Christian argumentation: "to scrutinize in detail" the postbiblical literature of Jewish exegesis "in the hope of neutralizing it or—even better—utilizing it" (p. 67). Friar Paul pursued this strategy in his famous Barcelona disputation of 1263 against Rabbi Moses Nahmanides, and, because of its initially mixed results, Friar Raymond refined it in his mammoth *Pugio fidei* (*The Dagger of the Faith*), from which Chazan took his book's title. Chapters 6 and 8 discuss the responses of Jewish writers to these two Spanish mendicants, rejoinders that, in Chazan's view, reveal both an awareness of the new Christian argu-

ments and an ability to deal successfully with them. The concluding chapter assesses the noteworthy influence of the new conversionist program on subsequent medieval polemicists, as well as its implications for an understanding of medieval Christian anti-Judaism.

The polemical sources considered in this work have been discussed at length by an array of modern writers, including Chazan himself, on numerous recent occasions; in light of the preceding summary, readers might be moved to question the novelty of the author's contribution. Never before has the phenomenon of thirteenth-century Christian missionizing among the Jews been the subject of a discursive, book-length review and analysis. Interspersed throughout this book, lengthy but lucidly translated quotations from the polemical works of the period convey much of the flavor and character of religious disputation in the Middle Ages. Furthermore, Chazan's study has led him to take issue with an interpretation that I have advanced during the past several years. In various contexts, I have argued that the spiritual climate of thirteenth-century Christendom in general, and the role of the mendicant orders therein in particular, gave rise to a new Christian ideological stance toward the Jews and Judaism. On the grounds that postbiblical, talmudic Judaism and its medieval proponents had forsaken the Mosaic covenant and therefore constituted heresy vis-à-vis biblical Judaism, Dominican and Franciscan friars concluded that European Jews no longer deserved the preservation that adherence to the Old Testament had warranted them in classical Augustinian theology. Acknowledging the innovative nature of the thirteenth-century mission as well as the decisive role of the friars, Chazan nevertheless denies that they manifested a new evaluation of postbiblical Judaism. The missionizing friars "made no break with prior conceptions of Judaism and the Jews"; they simply exploited hitherto inaccessible documents of rabbinic Judaism, which they still "assumed to be the continuation of Jewish misunderstanding of the covenant" (p. 175).

The constraints of a brief review hardly allow for a worthwhile reexamination of this question. But I would suggest that Chazan's denial of change in the theological estimation of Judaism among thirteenth-century missionaries remains unproven. His reading of the disputation of 1263, for example, may, as he suggests, be "fully plausible" (p. 173); his review of Martin's *Pugio fidei* may not have disclosed a new approach to postbiblical Jews. But, without such an approach, one is hard-pressed to explain the unprecedented and aggressive vigor of the church's mission to the Jews; indeed, its zeal ran counter to the earlier theology that Chazan insists explains it. The book offers no convincing explanation for the phenomenon that it describes so well, yet I believe that polemical and theological texts of the thirteenth century do.

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ROGER D. SORRELL. *St. Francis of Assisi and Nature: Tradition and Innovation in Western Christian Attitudes toward the Environment*. New York: Oxford University Press. 1988. Pp. viii, 204. \$24.95.

Roger D. Sorrell's book is the authoritative study of Francis of Assisi and nature in any language. It is scholarly but not dry, interesting without being anecdotal, challenging but not pedantic. Sorrell begins with the belief that there was no single medieval attitude toward nature before the time of Francis. Consequently, Francis was not a heroic lover of nature who bucked traditional attitudes in every way, but neither was he merely derivative. Sorrell carefully sorts out what Francis borrowed from a complex and multifaceted tradition and what was genuinely innovative in Francis's thought, deeds, and writings.

Sorrell examines a large body of medieval literature written before the thirteenth century, including monastic and ascetic works, saints' lives, patristic texts, and troubador poetry. He also carefully analyzes Cistercian contributions. Francis inherited a good deal from the ascetic and hermetical tradition, and much of his time during the first years of his religious vocation was spent praying and living in the wild. Francis, however, also inherited the apostolic and evangelical tradition of the gospels. Those two distinct traditions began to come together in 1213, when Francis was moved to preach to the birds at Bevagna. In the chapter devoted to that event, Sorrell makes clever use of the account in the *Fioretti* and of Thomas of Celano's *Vita Prima* to reconstruct the context and significance of the event.

Sorrell regards nature mysticism as Francis's "most untraditional reaction to creation" (p. 79), and he relates it to Francis's mysticism of the historical event, wisely following the work of Ewert Cousins. Three of the book's seven chapters are an extended commentary on and analysis of the *Canticle of Brother Sun*. Sorrell analyzes its sources, both sacred and secular, and accepts the account given in the *Legend of Perugia* of the circumstances in which it was written. Sorrell also looks at the *Canticle* in light of Francis's earlier *Salute to the Virtues* and *Praises before the Office*. The central argument concerns whether Francis was calling on the creatures to praise God or exhorting humans to praise God because God had made creatures such as the sun, moon, and water. By reading *per* in the *Canticle* as the Latin *propter* rather than as the French *par*, Sorrell convincingly argues that Francis was exhorting humans to praise God. For Sorrell, then, the *Canticle* is a call to people to appreciate God's creation and an implicit criticism of those who do not appreciate all that God made. (A crucial passage leading to that interpretation is *Legend of Perugia* 43). The *Canticle* is, he writes, "the highest poetic expression of an original Christian thinker" (p. 137).

Sorrell's entire argument is coherent and convincing. He relies on the work of Celano, Francis's own writings, and the *Legend of Perugia* more heavily than on the writings of Bonaventure for reasons he fully explains.

Two of the three appendixes about early Franciscan sources further explicate his emphasis on certain sources, although his views about the relationships between the sources will not please everyone. His analysis of Celano's *Vita Prima* 3 is the only unclear passage in the book. When he comments on the art generated by the Franciscan legend, he is not in command of the literature about the paintings and relies on outdated and unreliable scholarship. Had he examined the early representations of stories such as the sermon to the birds, he could have reinforced several points he makes in his argument. In an otherwise good bibliography, he has inexplicably omitted Eric Doyle's *St. Francis and the Song of Brotherhood* (1980).

Until now, the best study in English about Francis and nature has been E. A. Armstrong's *St. Francis: Nature Mystic* (1973). Although that book contains some valuable information not found in Sorrell, its argument is seriously flawed in a number of ways that Sorrell demonstrates both directly (for example, pp. 23 and 31) and indirectly. Sorrell's book is now the starting place for those interested in pursuing Francis's understanding of the natural world, and the introductory chapter on the tradition from early Christian times until the time of Francis is the best examination of early medieval views of nature.

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WILLIAM D. MCCREADY. *Signs of Sanctity: Miracles in the Thought of Gregory the Great*. (Studies and Texts: number 91.) Toronto. Pontifical Institute of Mediaeval Studies. 1989. Pp. xiii, 316. Paper \$32.00.

Gregory the Great composed his *Dialogues* in 593–94 to record miracles performed in contemporary Italy. William D. McCready's purpose in this study is to answer a relatively specific question (p. 3): "Did Gregory the Great really believe in the modern miracles recorded in the *Dialogues*?" He answers in the positive, emphasizing the important role in Gregory's thought that belief in miracles played.

The *Dialogues* constitute something of a historical puzzle, for in earlier writings Gregory seemed to claim that miracles had been necessary only in the early days of the church and had since ceased. Some scholars have claimed that Gregory underwent a change of heart on this matter similar to that experienced by Augustine. McCready begins by suggesting that Gregory never stated that the age of miracles had ended and that he regarded the miraculous as a consistently important element of Christian experience. Gregory did, however, think that miracles were relatively less common in "modern" times. McCready next considers the purpose and nature of such miracles. For Gregory they served, like the miracles of the New Testament, as an effective apologetic for Christianity. They could convince pagans or heretics of the truth represented by the Cath-

olic church. McCready stresses, however, that Gregory composed the *Dialogues* for a Catholic audience and intended them to provide inspiration in the crucial task of evangelization. Although miracles were performed by saints, they did not provide a sure indication of sanctity, for heretics, demons, and eventually the Antichrist could perform wondrous signs. The most important miracles were not those of physical power but those that involved moral virtue. To recount such exemplary stories was to be involved in the task of Christian pedagogy that animated so much of Gregory's work.

These initial four chapters provide a useful survey of the concept of the miraculous in Gregory's thought, providing comparisons to many writers, most particularly Augustine. In the following four chapters, however, McCready turns to the avowedly polemical purpose of establishing Gregory's belief in his stories. In a chapter entitled "Miracles: Fact or Fiction?" he counters the claim that many of the stories in the *Dialogues* were literary inventions directly borrowed from other sources rather than stories heard from informants. He considers a number of possible instances, most particularly those discussed by Adalbert de Vogüé in his edition (1978–80). McCready concludes that in all cases Gregory may plausibly have been passing on a story received from an informant, that in no case has invention been proven, and that, at worst, Gregory might be charged with being less than discriminating in his use of source material. McCready's arguments are not always convincing, and his analysis suggests that he would only accept verbatim repetitions as definitive proof of borrowing. He next summarizes the vast scholarly literature on medieval forgery with an eye to demonstrating that Gregory knew what a forgery was and condemned the practice. This summary leads to a statement characteristic of McCready's argument: "If our reading of the evidence is correct, and if we are charitable enough to reject the possibility of Gregory doing something that would have been dishonourable in his own eyes, there is one preferred conclusion: Gregory believes the miracle stories he tells, and is anxious that his readers believe them as well" (p. 175). From this point, McCready goes on to argue that Gregory's belief did not imply that he was of "a relatively low level of cultural attainment" (p. 177). Rather, he was a sophisticated reader and writer who viewed nature allegorically.

On balance, this is not so much a work of history as it is one of historiography. Although McCready has a thorough knowledge of Gregory's works, he is less interested in analyzing them from a fresh perspective than in criticizing earlier analyses. He quotes extensively from the works of other scholars and allows polemic, not exegetic, concerns to shape his argument. Much of his polemic seems curiously out of date. Most contemporary historians would not doubt that Gregory believed in the stories he recounted. Surely many recent writers have suggested that Gregory was not primarily concerned with historical accuracy, but just as

surely few today would take seriously the positivist approach espoused by Frederick Holmes Dudden in his *Gregory the Great: His Place in History and Thought*, published in 1905 and quoted here at length. Although McCready has certainly contributed to the lively debate over Gregory's purpose in writing the *Dialogues*, he has not adequately explored the question, more interesting to me, of what belief in miracles meant to Gregory and his audience.

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CHARLES T. WOOD. *Joan of Arc and Richard III: Sex, Saints, and Government in the Middle Ages*. New York: Oxford University Press. 1988. Pp. ix, 269. \$34.50.

The thesis of Charles T. Wood's comparative study of French and English government might not be guessed from the title. Although the book culminates in a detailed analysis of Richard III's demise, it has little to do with Joan of Arc and less with sex or sainthood. Wood deals with government, kingship, Parliament, and parlements but not with the gender history that his book's title implies. Wood is writing political history. Although he asks new questions of old material, he is basically theorizing about representative institutions.

In looking into how France and England diverged so far in their concepts of kingship, England developing a limited monarchy and France an absolutist one, Wood does indeed take the reader down some new paths. By stressing the differing solutions to such dynastic problems as royal minorities, he documents the importance of family history for understanding the divergence: French kingship was more strictly dynastic, defined by blood lines and hereditary right, whereas English monarchy was legitimated more through power and ability and by compromising with the barons and ultimately with Parliament. That fact made English kingship less secure but more flexible than French. It also made England safer for an adulterous queen: Isabella, after all, was not punished for her affair with Mortimer, but Louis X's adulterous wife, Marguerite of Navarre, was imprisoned and repudiated.

Wood challenges the claim that medieval people did not love their children by showing that Edward IV took great pains with the education of his three-year-old heir. But here a problem emerges: although it is admirable to ask new questions of political history, one must do more to answer them than give an example. Wood borrows questions from family history without submitting his material to its discipline. His results are so promising that one wishes he had done more, such as tracing male and female dominance, emotive relations, and sexual patterns through the two hundred years and several dynasties whose politics he has studied.

A lack of gender analysis also limits Wood's endeavor. He might have mentioned that Isabella, for example, was protected by the fact that her only child

was male and would therefore be the next king, whereas Marguerite of Navarre had only a daughter, who was eventually disowned along with the mother.

The lack of attention to gender is especially keen in the chapter on Joan of Arc. Unlike most historians of Joan, Wood gives her credit for affecting the future shape of the French monarchy. He writes, "[Joan's] impact on the political culture of France was immense, confirming and strengthening as it did the assumptions that had long shaped its governmental practices" (p. 145). Although Joan was ignorant of the practical politics of her day, she passionately believed that Charles had to be king because God had made him king. That idea had shaped French kingship before her time and would become the foundation of the divine right of later absolute monarchs. I suggest, however, that Joan accomplished her mission not because she was ideologically correct but because she used her mystical experiences to establish her authority in the world of powerful men, filling the accepted role of the female prophet who advised kings. Denying that Joan was either prophet or mystic, Wood does not ask how else a peasant woman could have risen to the top.

As for the much-argued legend of Richard III, Wood believes that Richard was handicapped by "a lack of imagination and foresight" and a "brilliantly limited" mind (pp. 179, 201) and that he got what he deserved. No one who violated the norms of his age as Richard did could survive the loss of trust from the baronage and Parliament. Wood's purpose in writing about Richard is to prove that Parliament had reached maturity and autonomy within the English system by the late fifteenth century.

Wood works close to his primary sources and is therefore interesting to argue with, whether one agrees with him or not. He does not, however, sufficiently explore what social history might tell us about the events he discusses. His book contains a number of fresh insights, but it remains too much a collection of essays (half of the text has been published previously as articles).

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SUSAN J. RIDYARD. *The Royal Saints of Anglo-Saxon England: A Study of West Saxon and East Anglian Cults*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 9.) New York: Cambridge University Press. 1988. Pp. xi, 340. \$54.50.

An account of the straightforward lines along which this work is constructed may, in a brief review, be the best way to give some idea of Susan J. Ridyard's achievement. After a preliminary statement of the problem implicit in her title, she lays out in splendidly comprehensible and analytical fashion the sources for each of the five cases she studies: those of Edburga (daughter of Edward the Elder), venerated chiefly at

Winchester and Pershore; Edith of Wilton and Edward the Martyr, both children of the glamorous late tenth-century monarch Edgar; a number of women grouped together as "the royal ladies of Ely," chief among them Aethelthryth (better known as Etheldreda), foundress and patron saint of the great religious establishment there; and Edmund King of the East Angles and Martyr, whose burial was the impetus for the grandeur of the monastic house eventually called Bury St. Edmunds. Then, after a chapter titled "Royal Birth and the Foundations of Sanctity: Theoretical Interpretations," she treats each of her chosen saints individually. A concluding chapter aims at an understanding of these royal cults, in the first instance by means of a distinction between those for men and those for women: "The cults of the royal ladies were products of the monastic world; those of the martyred rulers originated in the high politics of their former kingdoms" (p. 236).

This conclusion, though perhaps surprising, makes good sense in the context of these cults as Ridyard has traced them. She is clear-eyed about the distinction between sanctity and royal mystique, though a little less sure about keeping in mind how completely our view of the entire subject is seen through the filter of ecclesiastical writers, especially when those writers are hagiographers. To say that "the lives of the royal saints, it is clear, are wholly representative of early medieval thought on the nature of kingship" (p. 81) is to ascribe a more overarching value to these sources than many would think they deserve.

It is possible that the book would have been easier to use had the sections on the sources for each cult been included in the treatment of that cult. As it is, one needs to flip back and forth a good deal or to have taken very careful notes on the chapter on sources. For example, it is vital to remember her analysis of the purpose behind the composition by Osbert of Clare of a *Vita* of St. Edburga (succinctly expressed, "to provide the Pershore relics with a history" [p. 36]) when assessing the cult itself, which is not dealt with until many pages later.

That life of Edburga is supplied as a lengthy appendix (edited from Bodl. Laud misc. 114, carefully and helpfully described). There is a full and valuable bibliography and a detailed index. Virtually everything the author has done has been done well. Although the physical standards (paper and binding) are not what we have come to expect of this monographic series, the book is in every other respect an imaginative and distinguished work.

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RALPH V. TURNER. *Men Raised from the Dust: Administrative Service and Upward Mobility in Angevin England*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1988. Pp. x, 218. \$32.95.

The late twelfth and early thirteenth centuries were an age of increasing sophistication in government marked by a switch from memory to written records. Ralph V. Turner's thesis is that that phenomenon called for a new type of literate and numerative civil servant. Turner's general views are undoubtedly valid, although it is doubtful whether the advance of numeracy in any way equaled the advance of literacy. As late as the seventeenth century, standards of arithmetic were abysmally low even among men who could write the most elegant prose, and earlier generations had been even more inhibited by the mathematical limitations of Roman numerals.

The new civil servants of the period under discussion were often condemned by jealous contemporaries as "men raised from the dust"—a common condemnation of new types in all periods of history. From earliest times to the twentieth century, however, the upwardly mobile have usually come not from the dregs of society but from a class just below the class they have displaced in the hunt for jobs and money—a major reason for fierce resentment by the displaced. In this particular context of administrative development, other factors were very powerful: the facts that before the Industrial Revolution the quickest way to make a fortune was in the service of the state and that those who did so were by no means lily-fingered in their pursuit of profit. Turner might well have strengthened his case by making those points.

That being said, within the distinct limitations of the sources (they are very impersonal), Turner provides an admirable discussion of six of the most prominent of the new-style bureaucrats. Their careers certainly resulted from the need for greater professionalism, as there was no conscious royal bias against the magnates. Magnates wanted to be counselors, but they did not want the boring chores of day-to-day administration. Even so, they resented the influence and rewards of those who undertook those chores.

Of the six men studied, the two clerics, William de Sainte-Mère-Eglise and Henry of London, were both bishops. Little is known about their inner and spiritual lives, but they were certainly not mediocre (an accusation often leveled at the type in general), and it is arguable that their royal work made them better administrators for the church. Of the four laymen, one, Geoffrey FitzPeter, became an earl, and the other three, William Briwere, Thomas of Moulton, and Stephen Segrave, together with the bishops, all achieved at least baronial levels of wealth. They originated from no more than the lower and middling ranks of landholding families, and they were none too scrupulous about the acquisition of their wealth. It is clear from their activities that they had generalized rather than highly specialized talents in the modern sense, although Segrave, the latest of them, showed a greater tendency toward specialization. The laymen, though not without a certain military prowess, owed their importance to and practiced a wide variety of civilian skills in sherifdoms, land administration, financial

matters, and judicial activities. Their judicial activities were a result of practical experience rather than academic training. All represented prototypical professions, in fact, the precursors of the more fully professionalized types of the later Middle Ages.

Altogether this book is both informative and stimulating.

J. R. LANDER

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J. M. W. BEAN. *From Lord to Patron: Lordship in Late Medieval England*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1989. Pp. xii, 279. \$35.95.

The dispensation of and the search for patronage have been vital in all societies. Before the Industrial Revolution one of the major paths to prosperity for the upwardly mobile (to use fashionable jargon) was the resources at the disposal of the king and the landed magnates. J. M. W. Bean, already well known for studies on the decline of English feudalism and the fortunes of the Percy family, has now written a splendid volume, obviously the result of years of research and mature reflection, on the late medieval mutation of the form of patronage generally called (though mistakenly in the author's opinion) "bastard feudalism."

Investigations of this topic are notoriously frustrated by the disappearance of the bulk of the original evidence. Because the documents of bastard feudalism, unlike those of classical feudalism, dealt only with temporary contracts, there was little incentive for permanent preservation. Enough survive, however, to allow for the formulation of plausible hypotheses. Bean convincingly demonstrates that indentures of retinue began not in the fourteenth century but well back in the thirteenth and that their purpose, though partly military, was perhaps more importantly to restore the intimate personal bond that had been the essence of the first generations of classical feudalism but that had been seriously weakened from the early twelfth century by the growing independence of the mesne tenants as a result of the establishment of the principles of primogeniture and the hereditary tenure of fiefs.

Bean postulates (admittedly as a hypothesis) that the focus of the new relationship was still the aristocratic household (though it became less important by the late fifteenth century), that the value of the daily wages, liveries, and food there dispensed was for many retainers more valuable than the annuities that have tended to dominate previous accounts of the subject. Discussions of the developing legislation for the control first of liveries and then, considerably later, of retaining itself are stimulating and in general convincing, with totally justified warnings against taking the unusually well documented examples of John of Gaunt, the Percies, and Lord Hastings as typical, the abnormal extent of their retaining being a result of exceptional political circumstances and ambitions. Unlike the im-

mense expenditure of Gaunt and the Percies, few magnates spent more than one-tenth of their revenues on fees, and thus their limited followings hardly made them the overmighty subjects of legend.

Reservations occur here and there. I doubt, for example, the statement (p. 41) that Edward III exploited his subjects more than Edward I—rather the contrary, if bitter complaints about the ruinous effects of war taxation under Edward I are heeded. I am also dubious about the reliability of Simon Stallworthe's famous statement that all of Lord Hasting's men immediately after his death became the duke of Buckingham's men. It also seems to me that the political patronage of the crown was increasing well before the sixteenth century. A number of fifteenth-century magnates were anxious to get their hands on it to reward their own followers and thus to economize their own resources.

All in all, this is a most rewarding and stimulating work.

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RICHARD HOLT. *The Mills of Medieval England*. New York: Basil Blackwell. 1988. Pp. x, 202. \$55.00.

In the grain-fed Europe of the Middle Ages, mills were the one ubiquitous complex machine. Several aspects of mills in England are treated in this informative initial collection of data, which, regrettably, has been published well before its time. If Richard Holt has a thesis, it is M. M. Postan's old (and good) Malthusianism, with population pressures driving the history of milling. But then all of some and some of nearly all chapters lack relevance. If Holt seeks a broader understanding of medieval mills, this effort is inchoate and incomplete, a fragment about technology and society with little effective use of archaeology and not one thought about environmental impact.

From the outset Holt gives his audience neither path nor boundaries for the enquiry. A two-page preface describes research activities at Birmingham University conceived in 1983 by John Langdon and later taken over by Christopher Dyer and Holt. Other and projected publications by all three are remarked, although not how those relate to what appears here. Then Holt plunges into a useful demonstration that watermills had, by the time of Doomsday Book (1086), spread over much, but not all, of England. Windmills, however, were invented, probably in twelfth-century Western Europe and perhaps in England itself, to meet shortages of water power in flat and dry regions. The main diffusion of these came in the middle to late 1200s.

Four chapters address interlocking questions of profit and initiative in establishing and operating mills. Holt finds in English manorial mills more a token than a real instrument of seigniorial exploitation, if only

because the many independent mills then present show that peasants did value the work of the machine. But, after about 1250, returns to lords from mills rose along with the lords' power in a hungry countryside. Even the millers were visibly less the swaggering kulaks of Chaucerian myth than themselves victims of a growing lordly squeeze.

Holt's last four chapters succeed more as essays than as an integrated argument. Local and quantitative sources indicate that total milling power more than doubled from 1086 to 1350. Holt argues that technical innovations in the design of water mills were few and marginal, so "the windmill stands alone, as the only successful innovation in corn milling technology in the medieval period" (p. 144). He then takes issue with Eleonora Carus-Wilson and with Lynn White to minimize the importance of mills in industry before the sixteenth century. Medieval England had, in Holt's view, no "power revolution." After 1350 the profits of milling fell with the demand for cereals, bringing eventual dilapidation and abandonment of (especially demesne) mills. Disappointingly, Holt provides no conclusion. He detects signs of a sixteenth-century revival of milling, often with the cheaper horsemill, and stops.

Industriously collected data are no substitute for flawed presentation and thinking (not always to be distinguished by a reader). Holt brings much skill to quantitative texts such as the Hundred Rolls of 1279 but less to archaeological and technical information. Not once in his text, for instance, does he mention the splendid illustrations (pp. 6–7 and pp. 134–35) of mills reconstructed from excavated remains. Neither the temporal nor the regional limits of the work receives explanation. What grounds other than convention make the study end around 1500? Why make some comparisons with the European continent (for example, John Muendel's work on mills at Pistoia and Florence) and not others (for example, the well-known mills on the plan of St. Gallen)? Why, to be sure, is England the appropriate frame of reference to study this technology and its economic application?

Academic and professional libraries lack no candidates for their shrunken resources. Neither can long patronize purveyors of unripe and overpriced goods.

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ROLAND CARRON. *Enfant et parenté dans la France médiévale X^e–XIII^e siècles*. Foreword by PIERRE-ROGER GAUSSIN. (Travaux d'histoire éthico-politique, number 49.) Geneva: Droz. 1989. Pp. viii, 184.

Medieval childhood continues to attract the attention of scholars, who approach the topic from different perspectives. Roland Carron's monograph is a contribution to the history of the legal status of children in France between the eleventh and the thirteenth centu-

ries. The major sources are fourteen written customaries and some cartularies.

Legal sources are generally not a good place to look for traces of ordinary children; they had almost no independent legal existence and were absorbed into their families. Given the nature of his sources, the author concentrates on two kinds of extraordinary children: orphans and bastards. In chapters 3 and 4, he treats propertied orphans, because the customary law had virtually no interest in the protection of poor orphans. Even though Carron argues that the law gradually became more concerned with the child's welfare, the protection of a minor orphan's property loomed larger than that of his or her person.

The legal situation of orphans was dependent on their social status. In the eleventh century, an orphan who was heir to feudal property was placed under the control (*garde seigneuriale*) of his or her feudal lord. Such a lord had many ways to exploit the child's wealth, and families resented his threat to their patrimony. In Normandy, the dukes and their successors, the English and French kings, maintained the *garde seigneuriale* into the thirteenth century, but elsewhere in France customary law gradually gave the guardianship to an orphan's relative, usually the mother, although close paternal kin were often chosen. This institution, called the *garde noble* or *bail*, protected the child's property from ruthless exploitation and placed the child in the physical and moral care of relatives. Non-noble orphans with property were protected under an institution called the *tutela*, in which the kin had a major voice in the choice of the *tutor*, who was usually the mother or the uncle.

The other category of youngsters that interested lawyers was that of illegitimate children. Carron argues that, in the eleventh century, bastards were generally treated as members of their fathers' families, with rights to inherit. Subsequently, their situation deteriorated under pressure from ecclesiastical legislation and the strategies that noble families used to protect their property. The eleventh-century ecclesiastical reformers attacked the marriage of the clergy and vilified priests' offspring, whom they placed under serious disabilities. They were not attacking the illegitimate offspring of lay people, but their negative attitudes proved to be contagious. A powerful reinforcement (or perhaps the real motive) for the worsening status of bastards was the growing reluctance of noble families to splinter their inheritance. As such families began to favor the eldest son and to provide modestly for younger legitimate children, they excluded illegitimate children from a share in the family patrimony. In the thirteenth century, the situation of the illegitimate worsened as legitimate birth became a prerequisite for ordination to the priesthood and for entry to some cathedral chapters, monastic houses, guilds, and universities. By the late thirteenth century, the bastard was a social outcast, with no right to inherit from parents and subject to serious legal and social disabilities.

This monograph has certain strengths, in particular

its effort to cope with the enormous diversity of local custom in medieval France. It has more serious weaknesses, however. In virtually every quotation in Latin, there are errors that render the meaning obscure. On page 115, I counted ten errors in the Latin texts. There is also a noticeable number of misprints in French. The author was insouciant in his citation of sources, referring, for example, on page 30 to inadequately identified papal decretals. The author relied almost entirely on original sources, which could have been a strength but proved to be a problem because important modern scholarship on marriage, family strategies, and the status of children was apparently unknown, or at least unused. For instance, he did not cite such important works as Bernhard Schimmelpfennig's "Zölibat und Lage der 'Priestersöhne' vom 11. bis 14. Jahrhundert" (*Historische Zeitschrift* 227 [1978]: 1-44) or George Duby's many contributions to the study of French family structures, marriage practices, and inheritance patterns. On balance, this is a mediocre piece of scholarship.

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MICHAEL BORGOLTE. *Die Grafen Alemanniens in merowingischer und karolingischer Zeit: Eine Prosopographie*. (Archäologie und Geschichte, Freiburger Forschungen zum ersten Jahrtausend in Südwestdeutschland, number 2.) Sigmaringen: Jan Thorbecke. 1986. Pp. 341. DM 115.

Occasionally the early medievalist is troubled not by a habitual paucity of sources but by their overabundance. This is true for the history of Alemannia under Frankish overlordship in the eighth and ninth centuries, thanks to the enormous wealth of surviving charter evidence from St. Gallen. More than twenty-two thousand names appear in these texts, and even that number is only a portion of the whole, for there remain the *Necrologia* and *Libri Memoriales* of St. Gallen, Reichenau, and Pfäfers with thousands more. Michael Borgolte has found 783 mentions of counts, the foremost representatives of royal government. In more than five hundred cases, the formulas in which counts appear in charters also afford some insight into the local political structures and makeup of Alemannia. Borgolte's historical survey of Alemannic counts, *Geschichte der Grafschaften Alemanniens in fränkischer Zeit*, which appeared in 1984, explored this theme, and the present volume is an imposing prosopographical sequel. He offers no fewer than 115 studies of individual counts, their kin, and their connections and links with their rulers, based mainly on charters, buttressed by narrative sources if there are any. His entries are first and foremost references to comital gifts and gatherings of mentions in charters rather than biographies. Some counts were notorious as despoilers of St. Gallen, and we learn this fact mainly from unfavorable comments in the chronicles and *Vitae* rather than from charters, although the

same men might be donors as well and founders elsewhere. For knowledge of their mutual rivalries and struggles to dominate Alemannia, we depend on these isolated scraps.

Each entry is headed by a list of sources and a bibliography. Here an omission should be mentioned. In the articles devoted to a Count Albain and one of the Counts Bertold, Borgolte does not cite Donald Bullough's searching essay titled "Alcuin deliriosus Karoli regis: Alcuin of York and the Shaping of the Early Carolingian Court," published in *Institutionen, Kultur und Gesellschaft im Mittelalter: Festschrift für Josef Fleckenstein* (1984), pages 73–92, where Bullough shows convincingly that Charlemagne's ambassador to the Holy See in 773, mentioned in the *Liber Pontificalis*, was not Alcuin but a Swabian count. Alemannia was the region most closely tied to the heartlands of Frankish power. Alemannic nobles, such as Count Chadaloh and Scrot, entered into a closer partnership with the Carolingian regime than most, except for the Franks themselves. Those ties meant that the Alemannic nobles came to share the spoils and responsibilities of local comital power and of honors and rewards elsewhere. Among the men we meet in Borgolte's surveys are some of the foremost figures in late Carolingian East-Frankish military and political society, not least of whom were Erchangar II and Bertold IV, Burchard and Adalbert, all early tenth-century contenders for ducal overlordship. Borgolte's studies of individual counts and their families sometimes also reveal the very beginnings of countships in Alemannia in areas that had hitherto not known comital authority. When he unfolds dynasties that held office generation after generation, he does well in questioning the overconfident genealogies and chains of filiation much favored by an older generation of scholars. Early and high medieval Alemannia has been fortunate in its modern historians, thanks to Helmut Maurer's *Herzog von Schwaben* (1978) and Borgolte's studies. Here he has given us an austere but indispensable work of reference.

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DAVID ABULAFIA. *Frederick II: A Medieval Emperor*. Harmondsworth, England: Allen Lane; distributed by Viking Penguin, New York. 1988. Pp. 466. \$24.95.

The subtitle of David Abulafia's valuable study of Frederick II at once explains his approach and raises questions evocative of past controversies. Indeed, were it not for the disputes among the dead, the idea of Frederick as a "medieval emperor" would raise no hackles and would seem rather pedestrian. The subtitle does not, however, imply quite the contrast that some may think. Abulafia, whose extensive work in economic and social history has informed his approach to Frederick, is not really interested in the hoary debate between medievalists and modernists. His medieval

Frederick is very much a man living through a period of dynamic changes. It is the merit of this book that most of the old clichés, which presented Frederick as a figure of tolerance or as a beacon of rationalism, no longer occupy serious attention.

What Frederick emerges? Many will be dissatisfied at the lack of synthesis, at the almost tentative nature of many conclusions. Where Ernst H. Kantorowicz offered a poetic vision and Thomas C. Van Cleave a compendium, Abulafia points up directions in research and sums up the implications of previous studies as he sees them. There is room for some disagreement in this approach, but he does not bore his readers with an extended historiographical essay. He manages to keep Frederick very much at the center of things. The resultant image is perhaps unresolved but offers much for the thoughtful reader to digest.

Philip de Novare, whose knowledge of Frederick II was substantial and who wrote as a supporter of the Ibelins on Cyprus against the emperor's effort to conquer the island, provides an insight that may be helpful to the reader of Abulafia's book. The obsession of the medieval aristocracy with its rights, Frederick's own insistence on his rights (as he perceived them), the entanglement of law and order and rights, and even Frederick's conflict with the papacy form essential elements in a closely woven fabric of social relationships. From the beginning of his reign as king of Sicily, Frederick sought to assert his rights against Markward of Anweiler, the Sicilian barons, and the south Italian cities. His imperial coronation vastly expanded the sphere of rights that he had to assert and defend. No sooner had the imperial crown been won than he had to reestablish his position as king of Sicily. He had to continue the work of statecraft begun by his grandfather, Roger II, and the other Norman kings. At the same time, he was swept into the imperial role in Germany, in northern Italy, and as leader-designate of the crusade. Abulafia follows Frederick through these various activities. The diversity of Frederick's role in the kingdom of Sicily, communal Italy, and Germany raises difficulty for anyone searching for a unified explanation of Frederick's policies. It is precisely at this point that Frederick chiefly possesses the character of a medieval emperor. If his most visible achievements as a statesman and lawgiver are to be found in the kingdom of Sicily, particularly in the constitutions of Melfi, is this not precisely because of the foundations laid by his Norman predecessors? If his relations with the papacy and with the north Italian communes paralleled the frustrations of his grandfather, Frederick Barbarossa, was this not a reflection of the disintegration of the Lombard monarchy and imperial authority that had preceded either of them? In Germany, the ambitious policies of Frederick's son, Henry, aimed at securing effective royal power, threatened the support on which the Hohenstaufen as well as their Welf opponents had depended to maintain their hold on the elusive German kingship. If, in Abulafia's presentation, the various pieces seem greater than the sum of the parts, it

may be that he has exposed the weakness in earlier approaches that tried to make the parts add up as if they were a single sum.

This book is not an easy one to use. The decision to leave out footnotes will frustrate students not familiar with the literature. Its departure from familiar interpretations will arouse controversy. But it does offer a valuable new beginning in a field that has long needed one.

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T. A. PERRY. *The Moral Proverbs of Santob de Carrión: Jewish Wisdom in Christian Spain*. Princeton: Princeton University Press. 1987. Pp. ix, 198. \$28.50.

To the appreciation of the fourteenth-century Castilian Hebrew poet and Spanish moralist Shem Tov Arduziel (also Aduziel), T. A. Perry has contributed a trailblazing volume. Editor and commentator (1986) of Santob's *Proverbios Morales*, as the untitled work came to be called, Perry here presents the first full translation of the book into English and, accompanying it, a series of eight studies. The first three of these studies analyze what Perry regards as aspects of "Santob's self-perceptions as a Jew among Christians" (p. 7): his application of the imagery, from Song of Songs, of the thorn and the rose; his resort to metaphors of oppression and triumph as exemplified in a small but significant section of his work; and his triad of sins (envy, anger, and covetousness) against the backdrop of the seven deadly sins of the Christian tradition. The next three essays treat philosophical themes of concern to the medieval and Renaissance worlds in general and well represented in Iberian thought: the *mundo*, the world of matter and mortality, viewed with despairing resignation over its fickleness and chance; human society, regarded with skepticism, or, perhaps better, in Perry's felicitous term, "methodological doubt" (p. 116) over its deceptions in judgment and values; and the antidotal solution, in the form of correct ethics and behavior, centered on the golden rule in its reverse expression ("As you shall do, so shall it be done unto you") and a practical philosophy of life.

The final two studies consider the parallels between Santob's philosophy and that of Ecclesiastes and, under the title "God and Repentance," the question, answered affirmatively by Perry, of whether Santob can be regarded as a religious writer. Accompanying these studies is an invaluable appendix, containing an edition, translation, and discussion of the anonymous medieval prologue, originally accompanied by a commentary now lost and clearly deriving from a Jewish hand, found in the fifteenth-century M manuscript of Santob's text in Madrid's Biblioteca Nacional.

Perry's translation is remarkably faithful and lucid in its renditions, and his studies, though diverse, converge into the most comprehensive elucidation of Santob's work to date. Beyond their respective focuses, they

range over a variety of themes, many previously inadequately broached. They reveal a knowledge of Iberian, classical, Christian, and Jewish sources, a command of literary methodology, and a remarkable freshness of perspective. Above all, through Perry's detailed treatment and tantalizing hints, they set the direction for further investigation of critical elements of Santob's work: the eclecticism of his sources, secular Christian (from Greco-Roman classics to homespun wisdom) and not least Jewish, where many questions remain; the relationship of his work to his sociopolitical context, to which there appear to be both obvious and problematic allusions; and the social implications of Santob's construction of an apparently neutral, not primarily Jewish or Christian, and therefore really secular bridge between Jewish and non-Jewish thought.

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JAMES R. BANKER. *Death in the Community: Memorialization and Confraternities in an Italian Commune in the Late Middle Ages*. Athens: University of Georgia Press. 1988. Pp. xiv, 292. \$40.00

Seldom does a historian discover the type of magisterial contemporary source that is the crux of James Banker's important work on the confraternities of the Tuscan town of San Sepolcro. The author began with rich primary sources on the town's one great thirteenth-century confraternity and sources almost as excellent for its single most important fourteenth-century laud confraternity (*laudesi*). Even better, to locate these groups within the political universe, Banker had documents furnishing the names of the members of the political class to compare to those of the confraters. Then comes the crowning touch. Extant in San Sepolcro is an inedited work called the *Specchio*, written in 1437-38 by the confraternal notary and prior Francesco de Largi. Wanting to revitalize his own ancient confraternity, Largi studied the existing documents of at least two of San Sepolcro's confraternities. The *Specchio* is both a serial compilation of legacies left to the confraternities and a sophisticated evaluation of the moving forces in contemporary confraternal history.

An interpretive confraternal history of the Largi type is all but unknown in this age. In a skilled study of the primary sources at his disposal, critically crossed with Largi's interpretations, Banker, in a sparse 173 pages of text, furnishes the student of late medieval Italian social organization a marvelously concise survey of associative life in this town of four to five thousand inhabitants. In chapter 1, "The Topography of Worship," Banker demonstrates that there were relatively few clerics in San Sepolcro during this time, which largely explains for Banker why the laity was responsible for most "sacred" activity and signally the burial

and commemoration of the dead. In the next two chapters, Banker is concerned with the fraternity of San Bartolomeo first in the latter thirteenth century, when the group was very large and sustained by many small grants from its thousands of intra- and extramural members—grants that were promptly redistributed to the poor. Banker then considers the fourteenth century, when the membership shrank but the testamental grants of property became large, so that, in Banker's view, the group limited itself to administering these (land) grants. Chapter 4 studies a night laud company founded in the early fourteenth century; fascinatingly, its members were largely agricultural workers who, Banker believes, were the administrators of very substantial land legacies. The final chapter is a study of the small flagellant companies of San Sepolcro, which turn out to have had a large percentage of representatives from the town's political class. Last but not least, the publisher deserves praise for publishing in an appendix three Italian statutes of the fraternal, laud, and flagellant type. A selective bibliography and a fine index close the work.

This is a signal contribution to small-town Tuscan history, but Banker also puts solutions to some larger problems of Italian urban political life in closer reach. One problem is the role of confraternities such as San Bartolomeo—their membership as big as the town itself—as types of commonwealths before there were communes; thus, San Bartolomeo changed to a smaller group once San Sepolcro won independent status in the early fourteenth century. Another problem, which Largi grasped, is the crucial role that different lay religious groups played in recycling back into the economy partial and whole estates that testators, to save their souls while avoiding usury claims and other taxes, left for the Franciscans and others of the "poor." All in all, Banker's book is a solid and suggestive work.

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ANTONIO IVAN PINI. *Vite e vino nel Medioevo*. Foreword by VITO FUMAGALLI. (Biblioteca di storia agraria medievale, number 6.) Bologna: Cooperativa Libreria Universitaria Editrice Bologna. 1989. Pp. 204. L. 26,000.

In this collection of essays, Antonio Ivan Pini makes the point that Italy is the largest producer and consumer of wine in the world, yet there is no standard history of Italian viniculture. Pini's work, mostly devoted to Bologna and the Po valley in the Middle Ages, illuminates this important topic and makes a fresh contribution to the agrarian history of Italy.

The subject of wine is a good way to tie together various aspects of Italian culture. Wine was one of the few beverages of the Middle Ages, more satisfying and frequently healthier than water. Viniculture produced a food requiring intensive and skilled agricultural labor and a particular climate. The twenty-one different

types of grapes grown around Bologna testify to ingenuity in planting and grafting. The economic significance of wine in commerce is obvious, and Pini goes beyond this point to consider the importance of wine to culture. He observes that Christianity sanctified wine; its central role in the Eucharist meant that, wherever the religion spread, some attempt to grow grapes or to import wine followed. The arrival of Islam in some ancient areas of viniculture caused a reverse process by which the Mediterranean world developed a clear division into an area where wine could be holy and another where it was absolutely prohibited to the faithful. (In one essay Pini also discusses the olive, and some common themes emerge—delicate climatic requirements and a role in various sacraments.) Aside from wine's role in religion, Pini also stresses that it was one of the few means of escape from the trials of daily life. Merchants, peasants, and artisans, not able to lose themselves in noble pastimes like war, hunting, and jousting, might have found some solace in drink. Detailed tax records from Bologna reveal that its inhabitants in the fifteenth century drank an average of two liters of wine a day—more than twice the rate of consumption in Italy today—and favored red wine over white by two to one.

Viniculture had other significant effects on Italian society. Vineyards were valuable properties and became objects of investment, and Pini contends that they helped provide employment for rural people lacking the capital to enter the lucrative business of wine production. States and communes took an interest in the price of wine and found this commodity to be perfect for excise taxes. Viniculture illustrates how closely medieval cities were tied to their regions and how misleading it can be to study the two in isolation. After the golden age of wine production in Bologna in the thirteenth century, some vineyards were abandoned in the aftermath of the plague. Improvements in shipping before and after 1348 reduced the cost of transporting wine and other foodstuffs, and Pini believes Europeans gradually had a more homogenous diet. Cheap bulk trade and better barrels created a more international trade for wine, more refined tastes among consumers, and a fair amount of regional specialization.

This interesting and carefully researched collection of papers is, one hopes, a prelude to the standard history that Pini is the one to write.

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DONALD M. NICOL. *Byzantium and Venice: A Study in Diplomatic and Cultural Relations*. New York: Cambridge University Press. 1988. Pp. x, 465. \$59.50.

For some years various scholars have assumed that Byzantium and the medieval West (and sometimes medieval Islam as well) shared a single culture, whose essential unity has been obscured by modern bound-

aries between academic specialties. Such an assumption has lain behind a great many symposia, many articles, some general books, and frequent calls for further "comparative" and "innovative" research in this "neglected" field. It is therefore particularly surprising that no one before Donald M. Nicol has written a detailed study of the subject—Byzantine relations with Venice—that offers the best chance of supporting this assumption.

Venice had more in common with Byzantium than any other part of the medieval West. After evolving from a Byzantine province into an independent state over a period of centuries, Venice took the leading part in the Fourth Crusade's partial conquest of Byzantium and finally provided Byzantium's main defense against the Turks. Trade with the empire long dominated the Venetian economy, and Venice eventually became the main center for the transmission of Byzantine art and literature to the rest of Europe.

Given that the Venetian side has heretofore received more attention than the Byzantine, the best person to write this book was surely a senior Byzantinist such as Nicol, the author of a number of books and articles on Byzantium and its relations with the West. Like most of his books, this one is long but readable, thorough but free from unnecessary references and distractions. Unlike so much contemporary scholarship, it provides full and balanced coverage of a major subject rather than treatment of selected aspects. Although Nicol does not prejudge the question of how much Byzantium and Venice shared, he pointedly begins with contrasting epigraphs dated about 1200 from two Byzantines: the Emperor Isaac II pronouncing the Venetians "not foreigners but Romans from the beginning" and the historian John Cinnamus declaring them "morally dissolute, vulgar, and untrustworthy" (p. vi).

The book covers the period from the fifth century to the fifteenth, but its detail increases steadily as the sources become more abundant and relevant events proliferate. In this context of millennial development, Nicol shows how Byzantium and Venice exchanged their roles of world power and endangered city state and how Venice changed from the dependent of Byzantium to its enemy. He traces the growth of hostility between the two powers through the first four crusades up to the attempts of the Venetians to restore the Latin empire with which they had temporarily replaced and permanently damaged Byzantium. Finally, Nicol shows how common interests in the then-Balkanized politics of the Eastern Mediterranean brought Venice and the Byzantine remnant uneasily together again. In Nicol's account these changes all make sense, following from each party's utterly different needs, wishes, and outlook.

Misunderstandings naturally occurred but seem to have made little difference. Although the Byzantines' distrust of Westerners might have seemed ill-founded at the time of the First Crusade, later events fully justified their suspicions. The Venetians, too, had good reasons for their treatment of Byzantium, even for the

Fourth Crusade, which Nicol plausibly argues they planned to direct against Constantinople from the start. The Venetian technique of manipulating first the crusaders and then the Byzantines by means of debts to Venice that could not possibly be repaid was often reused in the subsequent period. Nicol understands but does not necessarily approve: "Venice can be charged with having milked Byzantium dry, though with the most lofty and legally unimpeachable motives of self interest" (p. 389).

In his preface Nicol summarizes the great differences in attitude between Byzantines and Venetians throughout their shared history, and in his concluding chapter he notes "the curious fact that, after so many centuries of close and often personal contact, there was so little fruitful rapport between Byzantines and Venetians, so little cultural exchange to produce a hybrid literature or art. Though frequently thrown together in Constantinople and Romania they kept their distance, as did the Greek émigrés to Venice" (p. 421).

Some may still think that this careful study somehow overlooks important elements that united Byzantium with the part of the West that was closest to it. A more probable conclusion would be that the modern scholars who try to demonstrate the cultural unity of Byzantium and the West are failing for the same reason that medieval scholars could not demonstrate the agreement of Plato with Aristotle.

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JESSE L. BYOCK. *Medieval Iceland: Society, Sagas, and Power*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xi, 264. \$32.50.

Jesse L. Byock has followed his first book, *Feud in the Icelandic Saga* (1982), with this wider-ranging study of medieval Iceland, which nevertheless remains firmly rooted in the author's own saga research. His basic premise is that the sagas are not just major literature but "the indigenous social documentation of a medieval people and, as such, they contribute a wealth of information about the functioning of a tradition-bound island culture" (pp. 49–50). In particular, "the family and Sturlunga sagas are a rich source of information on wealth and power in medieval Iceland" (p. 221), hence, this book's subtitle.

It is misleadingly claimed by Byock's publishers that he has used "all the available sources" to achieve his ends, although the evidence of archaeology is for the most part ignored by him. Alongside his clearly constructed guide to the Icelandic written sources (chapters 2 and 3), a similar introduction to the medieval archaeology of Iceland would have been of benefit to the nonspecialist reader for whom much of this book is intended, and that material could have been deployed to advantage in chapters 4 and 5, "Evolution of a New Society" and "Sources of the Chieftain's Wealth." Chapters 6 and 7 explore the development of Iceland's

consensual system of government and the role of the church. Byock comes into his own in chapters 8 through 11 in which his earlier observations are developed through a series of case studies from selected sagas that are examined in detail for information about social behavior and social patterning. Throughout the book there are numerous footnotes serving primarily as a guide to further reading; the references so deployed are also combined at the end to form a substantial bibliography.

Field survey and excavation are providing a clearer picture of the rapid exploitation of much of the Icelandic landscape, which led to farmstead desertion in many marginal areas during the medieval period with inevitable consequences for economic and social processes. Icelandic settlement patterns need to be examined in a wider, North Atlantic context, however, if conclusions of a deterministic nature are to be drawn from them (as Byock proposes), for dispersed farmsteads were as much the norm in Scandinavian Scotland as they were in Iceland, yet political developments were different.

Other archaeological avenues worth exploring include hoard studies, for the artifact evidence seems to be at variance with Byock's views on the availability of silver in Viking-age Iceland. Then there is the *hof* problem and Byock's silent rejection of Olaf Olsen's conclusions concerning the nature of this building for Norse pagan worship, although he cites Olsen's major interdisciplinary study, *Hørg, hov og kirke* (1966). Byock continues to translate *hof* as "temple," despite Olsen's demonstration that a *hof* would in practice have been a farmstead with a domestic hall above average in size in which neighborhood religious rituals (primarily feasting) took place. One has been excavated at Hofstadir in Iceland. A chieftain did not, therefore, have to maintain a separate "temple" in order to exercise his priestly functions; he could have used the neighborhood dues and offerings in support of his own establishment. And priestly duties need not have been of little or no financial advantage to a chieftain, as earlier scholars have concluded; they might have provided a means for Byock to reconsider in his pursuit of the system of chieftains' acquisition of wealth, a neglected area of Icelandic studies to which he directs attention.

To be just, it was Byock's purpose to extract such information from the written sources, using socioeconomic, rather than conventional literary, approaches. The originality of his insights, and the encouragement he gives to historians to return to the sagas as sources, should stimulate other researchers to investigate the important contribution that archaeology has to make to Icelandic medieval studies. Indeed, Byock fully recognizes the limitations of the written sources for an early history of Iceland, particularly for the initial settlement phase, beginning circa 870, and the period of paganism (to ca. 1000). This is the Viking age for which archaeology provides a rich, and growing, body of evidence

that should be of vital concern to socioeconomic historians.

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MODERN EUROPE

STEPHEN A. MCKNIGHT. *Sacralizing the Secular: The Renaissance Origins of Modernity*. Baton Rouge: Louisiana State University Press. 1989. Pp. xi, 131. \$25.00.

Historians of science, at least since the publication of Frances Yates's *Giordano Bruno and the Hermetic Tradition* (1964), have had to grapple with the issue of the role of Hermeticism and magic in the evolution of early modern science. Now Stephen A. McKnight turns to some of the same scholars who muddled the formerly positivist waters of the history of science (Frances Yates, D. P. Walker, Eugenio Garin, Paolo Rossi) and their sources (Marsilio Ficino, Giovanni Pico della Mirandola, Giordano Bruno) to argue that the Renaissance, with due respect to Jacob Burckhardt, and the scientific revolution did not secularize the European world. Rather, because of the neoplatonic religious mysteries of the Hermetists' "ancient theologians," they sacralized it. "Man the terrestrial God" (p. 3), deriving ultimately from the Hermetic corpus, was not, McKnight argues, devoid of the religious spirit.

Although McKnight's argument about the sacral nature of Renaissance neoplatonic thought will meet with general agreement, his further argument that this sacral quality persisted even in the most "modern" of modern thinkers probably will not. His thesis flies in the face of much modernization theory. One of the tenets of that theory, in its broadest terms, is that modern humans are secularized, the product of the Enlightenment and the scientific world view. Alex Inkeles's definition of modernity (*Becoming Modern* [1974]), for example, rests in larger part on the degree to which humans are secularized. McKnight believes that at the very heart of the secular is the sacral.

To make this interesting argument march, McKnight sacralizes Ficino and Pico (no difficult task given that the former was a priest and that both came under the influence of Savonarola) and Bruno (an ardent theist if not an orthodox Catholic). Contrasted with sacralists such as these are McKnight's villains, the arch-secularists Giovanni Boccaccio and Niccolò Machiavelli. But the real trick is his "reevaluation" of the three apostles of the modern age, Francis Bacon, Auguste Comte, and Karl Marx. To show the sacral nature of these three "positivist" thinkers would seem to clinch the case. Yet one doubts it. The book reminds one of Carl Becker's *Heavenly City of the Eighteenth Century Philosophers* (1932), a winning argument that *au fond* has something wrong with it. In Becker's case, it was that faith in reason was something qualitatively different from faith in the traditional God and revelation; in McKnight's case it is that, no matter what residues of the "ancient theology"

linger in Bacon, Comte, and Marx, these thinkers—especially the last two—are qualitatively different from Ficino, Pico, Bruno and the fabled Hermes Trismegistus. Overcoming alienation in the modern world is not the same as in the early modern world. To be *alienus* then was to be turned away from God; to be alienated today is a psychological problem and one that involves the reification of the human being.

In short, interesting as McKnight's book is, it is too slender to carry the weight of its arguments. The mere eighteen pages he devotes to discussing Bacon, Comte, and Marx as sacral secularists are too few to prove that point and to sustain his analysis of the Renaissance origins of modern sacral secularism. While brevity is always desirable, too little evidence cannot win the day.

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JOHN M. HEADLEY and JOHN B. TOMARO, editors. *San Carlo Borromeo: Catholic Reform and Ecclesiastical Politics in the Second Half of the Sixteenth Century*. (Folger Books.) Washington, D.C.: Folger Shakespeare Library and Associated University Presses, Cranbury, N.J. 1988. Pp. 323. \$39.50.

The essays in this volume were originally presented at a conference in Washington, D.C. in 1984, organized by the two editors to commemorate the four-hundredth anniversary of the death of Saint Carlo Borromeo. The opening essay sets the stage. In it, Eric Cochrane, to whose memory the volume is dedicated, sharply attacks what he considered stereotypical views of the "Age of the Counter-Reformation" and urges a more open-minded approach to the later sixteenth century in Italy. He preferred the term "Tridentine Reformation" to "Counter Reformation," since Counter Reformation is usually coupled with emotion-laden generalizations about its negative effects on Italian culture and thought that ignore new and creative elements. Cochrane's ideas are presented more fully in his posthumously published *Italy, 1530–1630* (1988) in which he certainly threw down the gauntlet to anyone tempted to repeat old explanations for the "decline" of Italy after 1527.

The following six essays focus on the figure of Borromeo and offer the reader a wealth of new ideas. Gone is the cardboard image of the saintly cardinal. Instead, we have a complex man who evolved from obedient papal nephew to reforming archbishop of Milan. Robert Trisco shows that the young Borromeo was by no means a consistent supporter of reform at Trent any more than he was above wheeling and dealing in papal power plays. Only after his ordination in 1563 and his increasingly serious commitment to the clerical vocation did he assume a more independent stance. John Tomaro points out that Borromeo, after taking up residence in Milan in 1566, clashed repeatedly with Pius V and Gregory XIII over jurisdictional

matters, especially the question of the nature of episcopal authority.

Here we come to the heart of the image of Borromeo that emerges from this volume. Saint Carlo, who chose to enter his see in episcopal garb rather than in the dress of a cardinal, saw himself primarily as a bishop and believed firmly that reform of the church could be advanced most fruitfully on the diocesan level. He fiercely defended his own position when Roman centralism stood in his way. Tomaro's essay argues that Borromeo envisioned some sort of federative church structure antithetical to Roman centralism, in which the episcopacy would play key roles in accordance with the Council of Trent's definition of bishops as agents of reform.

In Agostino Borromeo's essay, we see the archbishop insisting on the full exercise of his jurisdictional rights against Spanish authorities even to the extent of organizing his own police force, the *famiglia armata*. Saint Carlo and the king of Spain held opposing views of the proper relation of ecclesiastical to secular authority. Only gradually did Philip II come to realize that a strong leader such as Borromeo was not a dangerous rebel and that conflicts over precedence and jurisdictional matters did not preclude cooperation between church and state.

Adriano Prosperi examines the effects of Borromeo's activities on the laity. Saint Carlo was strongly opposed to the idea of an invisible church because he saw it as visible, ordered, and hierarchical. The magistrate rather than the martyr was his model for a bishop. The laity was to follow clerical leadership, authenticated by self-control and asceticism, in the never-ending war on this world.

That the severity of Saint Carlo was tempered by common sense becomes evident in the next two essays. John O'Malley looks at Borromeo's place in the history of preaching and argues that he was important as an intelligent preacher whose sermons were appropriate to his audience. He also promoted treatises on ecclesiastical rhetoric, such as that of his friend Agostino Valier, which determined the style of Catholic sermons for centuries. Paul Grendler deals with the Schools of Christian Doctrine in Milan and shows that Borromeo was able to transcend his prejudice against lay teachers who did the catechetical work of the church among the poor and offered instruction in reading and writing.

The second group of essays is devoted to Borromeo's influence. Beginning with his detailed instructions concerning church buildings and furnishings, presented by E. Cecilia Voelker, we see his pervasive impact on Catholic Europe. A. D. Wright draws parallels between Borromeo's reform ideas and those of Spanish clerics. Marc Venard shows the importance of Borromeo as model bishop for early modern France because of his rigorism, his organized, bureaucratic pastorate, and even his promotion of the use of the confessional. John Headley discusses the "dreadful challenge" that Tridentine reform presented to established social and economic privilege in Germany and focuses on Bor-

romeo's intimate friend and papal nuncio Giovanni Francesco Bonomi as an exponent of that reform.

Borromeo as model is the theme of Giuseppe Alberigo's essay. He thinks that the saint's decision to become a resident archbishop, who preached, convoked synods, and exercised his office in a personal way, was his true "conversion." Borromeo became the incarnation of the Tridentine idea of the bishop. But that image was played down by Rome. Niels Rasmussen discusses the ritual of San Carlo's canonization in 1610 and points out that ironically it was Borromeo the cardinal of the Roman church rather than the bishop of Milan who was exalted.

Alberto Melloni brings us to the present by studying the influence of Saint Carlo on Pope John XXIII. He traces the development of the young Roncalli's historical consciousness through work on the documents of Borromeo's pastoral visitation of Bergamo and argues that Roncalli came to see the famed harshness of the great archbishop as a historiographical myth. In concluding the volume, Carlo Marcora calls attention to the voluminous materials on Borromeo in the Ambrosiana Library of Milan.

These essays are remarkable for their high level of scholarship and their imaginative sweep. Collectively, they give impetus to a new point of departure for studies not only of Borromeo but also of the period that Eric Cochrane would have liked to see called "The Age of Consolidation."

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PIER PAOLO VIAZZO. *Upland Communities: Environment, Population, and Social Structure in the Alps since the Sixteenth Century*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 8.) New York: Cambridge University Press. 1989. Pp. xv, 325. \$49.50.

Pier Paolo Viazzo's book is a study of relationships among population, environment, and culture in the alpine area, concentrating on the period from the eighteenth century to the present. It analyzes regional and international patterns and focuses on a case study of one small, German-speaking village (Alagna) in Piedmont. Viazzo, an anthropologist, addresses several theoretical problems lying at the intersection of population ecology, human geography, and social anthropology. In particular, he examines whether and how "upland" populations have historically achieved a balance between their numbers and their economic resources.

The author devotes the first three chapters of the book to an exegesis and critique of previous works on population and environment in alpine and other upland communities. Although this discussion is probably critical to his primary audience, historians may find it too long and detailed. In it, Viazzo challenges the work of population ecologists and other scholars who have,

in his judgment, argued too strongly for the primacy of physical environment in shaping and somehow standardizing cultural rules of upland populations. He also disputes models of the population-resources relationship based on an assumption of upland communities' inherent poverty.

Beginning in chapter 4, Viazzo addresses central issues of alpine demography, starting from the twentieth century and working backward. He discusses the "demographic transition" in different subregions in the twentieth century and shows the importance of high ages at marriage in a number of villages since the mid-nineteenth century. Viazzo also shows that overall fertility has been low in many parts of the Alps since that time. These findings lead to the subject of migration, the most interesting part of the demographic discussion.

Viazzo challenges the "safety valve" theory of migration, which posits that out-migration from upland communities has been the principal strategy used to maintain a livable balance between population size and economic resources. On the contrary, his evidence from local and regional data on fertility and nuptiality points to a "low pressure" demographic equilibrium, created by controls on both. Thus, from the eighteenth century until World War II, seasonal emigration of Alagna's men was not a poverty-driven solution of last resort to imminent structural overpopulation but a rational and highly successful search for opportunity in lowland economies. In many areas, this seasonal migration, which was most characteristic of communities at the highest altitudes, led to the paradox that high-altitude villages were frequently more integrated into regional economies and the wider society than villages at medium altitudes. Alagna's own economy was organized around a system of labor division in which agriculture was the work of women and male immigrants.

The historians among Viazzo's readers may wish for more discussion of the quality of the sources used in his family reconstitution and of the demographic sources in general. They should be pleased, however, by the author's conclusion, namely, that despite similar physical environments and frequently similar demographic regimes, alpine populations display those diverse cultural responses to material conditions that historians and anthropologists alike still delight in studying. The book as a whole is designed to be broadly comparative. Its attention to issues of theory, its coverage of a broad interdisciplinary literature, and its findings about the "preventive checks" used to balance population and resources should make the book of interest to scholars studying upland communities throughout the world, from the Andes to the Himalayas.

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PAUL SONNINO. *Louis XIV and the Origins of the Dutch War*. (Cambridge Studies in Early Modern History.)

New York: Cambridge University Press. 1988. Pp. xii, 226. \$39.50.

On March 28, 1672, Charles II, king of England, Scotland, and Ireland, suddenly declared war on the Dutch Republic. "No clap of thunder in a fair frosty day could more astonish the world than our declaration of war against Holland," wrote an English minister. But it did not astonish Louis XIV, king of France, for on April 6—as soon as news arrived of the English action—he followed suit. Why? The war was certainly neither necessary nor inevitable. France was in no way threatened by the Dutch, and what Louis really desired was the conquest of the Spanish Netherlands, which he had attempted in his last war (1667–68) until thwarted by the Triple Alliance formed by Charles II, the Dutch Republic, and Sweden. But then, in spring 1669, just as Louis and his ministers were planning their next move, Charles II changed sides. He communicated to Louis his desire to declare himself a Catholic and to introduce toleration for Catholics in his kingdoms, and, in return for French support in that endeavor, Charles offered a full defensive and offensive alliance. Frustrated in his principal foreign policy goal, Louis therefore decided to attack the Dutch. He hoped that the Spaniards could be prodded into intervening, thereby offering their south Netherlands possessions as a sacrificial lamb for his armies to dismember and devour. The Treaty of Dover was signed on June 1, 1670. At first, war was intended for 1671, but crises first in Lorraine and then in the Rhineland diverted France's forces so that the declaration of war was delayed until the spring of 1672.

Paul Sonnino's account ends as the armies and fleets moved into action, for his purpose is not to chronicle the war but to explain exactly how Louis XIV and a small minority of his ministers plotted and planned to secure the war they wanted. Sonnino argues that the Dutch War was never popular—that, on the contrary, many both in the country at large and in the highest echelons of government disapproved. Some of the book's most interesting passages concern the ways in which Louis and his "hawkish" advisers managed either to neutralize or to win over their opponents. "The origins of the Dutch War," Sonnino writes, "provide another depressing illustration of the manner in which men of power are able to appropriate the consciences of men of talent" (p. 192). To make his case he has ransacked the archives not just in France and England but in Austria, Belgium, Czechoslovakia, Italy, East and West Germany, the Netherlands, Spain, and Sweden. Where the records of the French government are silent, Sonnino has consulted the reports of foreign ambassadors, and, to assess the international response to Louis's various political initiatives, he has examined the records of the major foreign governments. For example, he demonstrates from French sources that Jean Baptiste Colbert (Louis's chief financial adviser) was totally opposed to the Dutch War until early November 1671. And from the dispatches of the Italian ambassadors, Sonnino proves that Colbert's capit-

ulation occurred during an interview in which Colbert said he did not see how he could finance the proposed campaign, and the king retorted, "Think about it. If you can't do it, there will always be someone who can" (p. 172). The majority of the sources cited in this book are French, but careful examination of the full range of the available evidence reveals much that is new. Above all, Sonnino restores the *Mémoires* of Louis XIV to their rightful place as a prime source on the king's inner motives for his major policy decisions.

This study will likely become not only the definitive work on its subject but also a model for other works of diplomatic history. I have only two complaints. First, Sonnino's prose is sometimes overexuberant. For example, Sonnino describes a meeting of some minor German princes in September 1669 in the following terms: "Gravel was in such a tizzy over John Philip's nefarious proposals at the Imperial Diet that Franz and Herman could afford to play their parts to the hilt" (p. 80). And he heralds the news that Charles II was willing to make a deal with France the following month as "the most earthshaking *mémoire* of the century" (p. 84). Second, Sonnino includes too much extraneous material on the daily routine of court life: what the king did, where he went, and with whom he spent his time are discussed in detail irrespective of the impact of those activities on foreign policy. We are offered, for example, a detailed account of Louis's reproach to his homosexual brother in May 1670. The king justified sending Monsieur's catamite into exile with the words, "I have to correct your faults!" "If it's a matter of faults," replied Monsieur petulantly, "what about yours?" "Mine are with women," the king explained, "yours are an abominable vice" (p. 111). It is hard to see how that exchange, interesting though it is, affected the origins of the Dutch War.

Even when its relevance is questionable, however, the material is always interesting, and most readers will feel that the vignettes on life and love at the court of the Sun King are well worth reading. Sonnino presents more than enough explicit material to show why, in April 1672, Louis XIV was able to lead his forces out on what he thought would be a short, sharp, glorious campaign to panic the Dutch into making humiliating concessions—and why he was so utterly mistaken.

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KARL W. SCHWEIZER. *England, Prussia, and the Seven Years' War: Studies in Alliance Policies and Diplomacy*. (Studies in British History, number 14.) Lewiston, N.Y.: Edwin Mellen. 1989. Pp. ix, 325. \$59.95.

This volume of essays is a comprehensive, significant contribution to studies of the Anglo-Prussian alliance of 1756–62. The collection was inspired by Frank Spencer's proposal for a fresh and more detailed approach to the brief, shaky history of the coalition that

linked Great Britain's wartime ministers with Frederick the Great, Europe's consummate autocrat of the era of the Seven Years' War (Frank Spencer, "The Anglo-Prussian Breach of 1762: A Historical Revision," *History* 41 [1956]: 100–12). Karl W. Schweizer explains his effort as an "integrated reassessment" of the alliance's stormy existence in the light of his study of official and private documents either unavailable or unknown to earlier scholars. The author's nine essays present fresh, well-documented interpretations of episodes linked with the origin of the alliance in 1756 and its demise in the late war months of 1761–62. The result is a thorough, up-to-date, and adroit study of the subject that scholars are well advised to consult.

Schweizer says that the great power alignments of the War of the Austrian Succession were undone by that struggle's creation and excitation of issues requiring basic changes in international relationships, a "structural crisis" that bred the coalition of the Hanoverian and Hohenzollern dynasties. Britain was bidding for maritime, commercial, and imperial supremacy over France beyond Europe while needing an ally to secure Hanover within Europe. Frederick II, whose seizure of Silesia signaled Prussia's ascendancy as a great power, needed an alliance with England to avoid isolation in face of a continental coalition evolving from the diplomacy of Austria's Anton von Kaunitz. Schweizer forcefully argues that this Anglo-Prussian marriage of convenience, consummated by the Westminster Convention early in 1756 and the Subsidy Treaty of 1758, was destined to a short, tempestuous existence because of dissimilarities in the allies' basic war aims and opponents from the outset of the war. The shifting sands of fortune and circumstance between 1756 and 1762 augmented the instability of an alignment temporarily sustained by William Pitt's strategy to win America on German battlefields and Frederick's growing need for British continental commitments until his deliverance was wrought by Peter III's stunning reversal of Russian policies in early 1762.

Five essays focus on the critical period from the rise of the earl of Bute in October 1761 through the Subsidy Treaty's termination in the spring of 1762. Schweizer reexamines the story of this turbulent juncture by analyzing the text of Frederick's *Projet de Convention Eventuelle entre leurs Majestés Prussiennes et Britanniques*—a document hitherto unknown to scholars—and concludes that the king, during the weeks prior to Peter's succession in Russia, put the Bute ministry in an untenable position by exacting, as the price for approving British peace negotiations with France, unrealistic British commitments on the continent that English opinion, official and otherwise, opposed on strategic, financial, and political grounds. Both before and after Peter III's *volte-face*, Frederick's charges of English duplicity and betrayal were amplified by independent British contacts with Austrian officials at the Hague and Frederick's reception of the prejudicial views of Prince Golitsyn, the Russian ambassador in London, on Bute's diplomatic intentions.

Reassessing the impact of the Hague overtures and Golitsyn's influence on the allies' relations in early spring 1762, Schweizer concludes that, notwithstanding Bute's arrogant tactlessness, his actions were rooted in a tenacious pragmatism toward British national interests in circumstances that would have taxed the abilities of a Pitt or Churchill. Frederick's conviction of Albion's perfidy, colored by his impatience with its parliamentary politics and insular evasion of continental commitments, caused him to have no compunctions in his own secretive dealings with his new Russian ally—conduct that animated Bute's distrust of the king and the end of the subsidy on April 30, 1762. Political and psychological exigencies bred by late wartime circumstances moved England and Prussia toward peace settlements in 1763 reflecting geopolitical divergences that had existed from the conflict's beginning.

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LAWRENCE C. JENNINGS. *French Reaction to British Slave Emancipation*. Baton Rouge: Louisiana State University Press. 1988. Pp. ix, 228. \$27.50.

Although British emancipation (1833–38) had ramifications throughout the Atlantic slave world, major attention has thus far been focused on the Anglo-American connection. Lawrence C. Jennings's book is the first monographic study of the French response to British emancipation during the final years of France's second slavery (1802–48). More thoroughly and comprehensively than ever before, Jennings shows how the British example initially stimulated French consideration of abolition through anticipations of revolutionary contagion and strategic threat. By the 1840s, however, both the economic results of "the Great Experiment" in the Caribbean and British anti-slave trade activities provided increasing ammunition to French interests wishing to decelerate the movement toward liberation. Jennings richly details how colonials, bureaucrats, and journalists were able to focus attention on policies and outcomes that increasingly immobilized French abolitionists. Only the Revolution of 1848, concludes Jennings, reversed a situation in which British policies impeded French action.

If I have any reservation about Jennings's analysis, it derives from his less than systematic treatment of the economic impact of the British experiment. Because he does not clearly insert his findings about the French debates into the general political economy of Atlantic slavery, Jennings's evaluations of the impact of British colonial developments on the French debates sometimes blur the distinction between distortion and realistic description. Initially characterizing the French emphasis on problems of falling production and withdrawn labor as examples of class or colonial bias, Jennings eventually concludes that French official and plantocratic perceptions accord with recent historio-

graphic assessments. Near the end of his study (p. 204), Jennings expresses amazement that the British experiment was not used more often or more systematically by abolitionists in the period just before 1848. He ascribes this reluctance to Franco-British clashes in the early 1840s. But why, in that case, did the references decline most dramatically after 1845, when both the war fever and the right-of-search quarrel had subsided? The "waning" of the British example coincides more precisely with the colonial distress that followed British abandonment of "free sugar" protection in 1846. For the *colons* of Martinique on the other hand, the "disaster" of the English solution (p. 194) was as relevant to their brief in 1848 as it had been a decade before.

And, as the study implies, the economic dimension of emancipation was not foisted on abolitionists by their opponents. It grew directly out of more than half a century of British abolitionist propaganda. When Jennings criticizes the French for failing to respond effectively to economic arguments, he does not make it clear that French abolitionists lacked the "moral" constituency with which the British had been able to override appeals to economic criteria.

Apart from the uncertain integration of economics, Jennings's account is quite consistent with the recent historiography of Atlantic abolitions. The elitism of French abolitionism narrowed both their grounds of debate and the available political options. This study amply demonstrates that, if British emancipation set the clock running on France's second emancipation, it also contributed to the temporizing propensities of the July Monarchy's *grands notables*. One confidently looks forward to Jennings's sequel on French emancipation itself.

SEYMOUR DRESCHER
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JUDITH E. ZIMMERMAN. *Midpassage: Alexander Herzen and European Revolution, 1847-1852*. (Russian and East European Studies, number 10.) Pittsburgh: University of Pittsburgh Press. 1989. Pp. xvi, 305. \$39.95.

The revolutions of 1848 began in hope and ended in disaster for the radicals of Europe. Atop revolutionary barricades from Paris to Berlin, Vienna, and Naples, working men and women proclaimed their independence and freedom between February and May only to see their triumph crumble as the forces of order returned to crush them between June and December. As revolutionary leaders exchanged the glory of the barricades for sentences of exile, prison, or death, realism replaced idealism in politics, and less volatile, more sober folk took up the cause of Europe's proletarians.

Few were more bitterly disappointed in the revolution's failure than Alexander Herzen, the Russian nobleman turned socialist who had come to Europe to criticize, as Zimmerman notes, "the bourgeois society

which he had never seen, but which his reading of Proudhon and Blanc had taught him to despise" (p. 39). The revolution's failure and the victory of the bourgeoisie strengthened Herzen's conviction, Zimmerman adds, that "the entire civilization of the West was rotten and would be swept away by the triumph of the proletarian 'barbarians'" (p. 89). This would shape his view of Russia's role in future upheavals. For Herzen, pessimism about Europe stirred brighter visions for revolutionary Russia.

For years, Herzen has been perhaps the best known of nineteenth-century Russia's revolutionaries in the West thanks to his own writings and to the brilliant portraits of him that have been drawn by Edward H. Carr and, especially, by Martin Malia, whose magisterial study of Herzen's psychological make-up and intellectual development has dominated the scholarly landscape for nearly thirty years. Surprisingly—at least to one who used to think that little could be added to these earlier accounts—Zimmerman's book further broadens our understanding of Herzen's life and development during the crucial years that spanned his coming to Europe in 1847 and his settling in London (where he soon would establish the nineteenth-century's most influential émigré Russian press) in 1852. Zimmerman has explored the intricacies of Herzen's European associations meticulously. The result is a careful recreation of the society with which he surrounded himself that places Herzen more firmly and more clearly into the context of Europe's revolutionary movement. On that score, the results of Zimmerman's thorough examination of archival sources that extend from Paris, London, Liestal, Manchester, and Amsterdam to New York City will not soon be superseded.

Yet, on another level, Zimmerman's account is less satisfactory. Herzen was a passionate, dedicated man in search of a future whose vision he glimpsed only imperfectly. He lived in turbulent, tumultuous times when people hoped to transform the present to fit such visions, and he lived a life full of triumph, tragedy, and crisis brought on by broken dreams, shattered hopes, betrayals of friendship, and the loss of family. Between 1847 and 1852, Herzen abandoned his homeland and the close circle of friends around which he had built his life in Moscow and began five years of wanderings across Europe as he searched for new moorings in the West. While he did so, death claimed his mother, his deaf son, and his wife, whose sexual infidelities with his close friend Georg Herwegh had caused him much self-doubt and personal pain. One need only read Herzen's autobiography, easily one of the greatest ever written, to sense the breadth of his passion, the heights of his exhilaration, and the depths of his disappointment during these years. Yet there is neither agony nor ecstasy in Zimmerman's pages as Herzen moves across them. This does not detract from the scholarly worth of the book, but it weakens our sense of Herzen, the man and revolutionary, who so often seemed to find the best in himself in the company of others. One can hope that, if Zimmerman carries her study further, a more

complete Herzen will emerge from her continued efforts to integrate his story with that of the European revolutionary movement.

W. BRUCE LINCOLN
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MODRIS EKSTEINS. *Rites of Spring: The Great War and the Birth of the Modern Age*. Boston: Houghton Mifflin. 1989. Pp. xvi, 396. \$24.95.

Intuition tells the historian of contemporary culture that there must be a connection between our century's outrageously rebellious arts and its shocking outbursts of barbarism. This is the intuition that has lured Modris Eksteins into this epic of a book, with a cast of thousands and a tumult of facts. He has chosen *The Rites of Spring* as both a paradigm of modernism and as his title. Eksteins argues that this 1913 ballet—its atavistic music by Igor Stravinsky, its luxurious setting by Léon Bakst, its erotic choreography and performance by Vaslav Nijinsky, and its notorious production by Serge Diaghilev—has a special relationship to the war that broke out a year later. The primeval blood sacrifice in the art symbolizes a set of cultural values that carried over, he argues, into the barbarity of the trenches and, indeed, as far as the apocalyptic rhetoric and deeds of Adolf Hitler and his followers.

A plethora of evidence in the book supports this argument. The large amount of source materials available to twentieth-century cultural historians, however, presents abundant evidence for many other plausible interpretations. The contemporary historian must refine gold from a gargantuan slag heap of material: "mass" is the key word of our time in many respects—media, culture, ideology, and, alas, hysteria.

Eksteins certainly has gathered a mass of quotations, descriptions, moments, facts, and people. Profligately he drops names: Isadora Duncan and Mabel Dodge, Karl Kraus and Fritz Kreisler, Franz Kafka, Fyodor Dostoevsky, James Joyce, and T. S. Eliot; they pile onto the hordes of nameless soldiers at the front and wives pining at home. But, instead of coalescing into a Greek chorus, they rush past the reader like a mob of doughboys charging into no-man's land. Ultimately their existence is as insignificant as death on the Western Front.

The author's restless camera roves across Europe. The prologue, for example, sensitively discusses the mystical power of Venice over the early twentieth-century creative spirit. But Venice never reappears after page five. Then it's on to Paris for a breathless dash through secondary sources on the opening of the *Sacre du printemps*. By page fifty-five, we are in Berlin, where Kaiser Wilhelm displays warm affection for theater and dance and astounds aides with his intimate knowledge of same. We learn that his general, Helmuth von Moltke, painted and played the cello, was said always to carry a copy of Johann Wolfgang von Goethe's *Faust*, and was working on a translation of

Maurice Maeterlinck's *Pelléas et Melisande*. On the next page, these two men act on perceptions of "aggressive designs and ambitions of the hostile powers surrounding Germany" (p. 90) by launching aggressive war.

Indeed, war in 1914 was "in the air," and the outbreak was greeted on all sides with an orgy of strident rhetoric by otherwise cultivated people. But this book never pins down the connection between the art that punched through traditional boundaries of taste and the actions that destroyed all previous norms of Western civilization. In their cultural bent, the Kaiser and von Moltke personify the century's burgeoning audience for the arts. But they also personify that audience's predilection to treat the arts as entertainment, as diversions from serious professional, political, social, or economic concerns, and as an enhancement not of inner life but of outer image. Bestiality hides in a cloak of culture: the concentration camp commandant assembles a string orchestra from among his victims.

As if the book's vast scope of time and place were not challenge enough, Eksteins also addresses his study to an indeterminate audience, a mass best guessed to comprise the *Kultur* mavens who attend museums and applaud every *tour jeté* at the ballet. Hence, the author must explain and describe society, economy, politics, and armies in France, Germany, and Russia over a span of half a century. This mire of factual background fatally engulfs argument and conclusions. Sometimes, too, the facts do not support the argument. Homosexuality, for example, may well have been more acceptable in pre-World War I Berlin than in Paris or London, but the presence of forty bars that catered to homosexuals and one thousand male prostitutes may have meant only that homosexuality was more overt in Berlin than elsewhere. Certainly, the French were more sexually emancipated, and in Britain, Oscar Wilde's crime was not so much in what he did but in what he publicly said.

Eksteins's study is not a slapdash book; rather, it suffers from an excessive period of gestation. One senses theses explored, flowered, and then gone to seed, only to spring up during the next season's research campaign, choked by weeds. That art is a surrogate religion in our century and that war is hell are hoary clichés. The intricate web of connection, tantalizing as it is, ultimately eludes this author.

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ZEEV STERNHELL *et al.* *Naissance de l'idéologie fasciste*. (L'espace du politique.) Paris: Fayard. 1989. Pp. 424. 140 fr.

Zeev Sternhell attributes the birth of fascist ideology to three main elements: a Social Darwinist tribal nationalism, an attack on liberal democracy, and an antimaterialist revision of Marxism. On the last point, his

analysis is strikingly contradictory. He sometimes portrays the revision as "not" a "variety of Marxism" or its "shadow" (p. 12), then as one that "totally modified" the "nature of Marxism" (p. 336), "emptied the [Marxist] system of its original content" (p. 32), and strongly supported free enterprise capitalism (pp. 16, 39, 306–07), yet at other times he calls the revision "a new variety of socialism," a "synthesis" of Marxism and nationalism (p. 25), and one that was "cut off" from conservatism and acted as an "unprecedented war machine against the bourgeois order" (pp. 45, 305, 317). According to Sternhell, this explains why "so many" men of the left, both before and after the First World War, "slid toward fascism" (p. 45).

Sternhell attempts to resolve the dichotomy by emphasizing that fascism sought not an economic but a cultural revolution, that its national "socialism" stopped at psychological and aesthetic goals. For Sternhell, Georges Sorel was one of the major figures behind this "revolution," an enemy of "bourgeois values" who, along with such writers as Filippo Marinetti, Vilfredo Pareto, Ezra Pound, Wyndham Lewis, and Benito Mussolini, rejected Marxist materialism and determinism, Enlightenment rationalism and democracy, and liberal individualism and universalism in behalf of an avant-garde cult of vitalism, will, irrationalism, elitism, nationalism, and violence. Revolutionary syndicalists, confronted with the failure of Marxist theory (the poor had grown richer under capitalism, and one could not count on economic determinism to bring about a revolution), concluded that only a "heroic" elite, imbued with the military values of the First World War and supported by a proletariat mobilized not by dialectical materialism but by social "myths," could bring down the established order.

Yet Sternhell, in relating the "sovereign scorn for bourgeois values" of these revisionists and their desire to "vomit" the established order (p. 337), fails to call attention to the obvious: these intellectuals were not opposed to all bourgeois values by any means and certainly had no desire to vomit the whole of the established order. They were highly selective "revolutionaries" who had no quarrel with the capitalist structure of society and the social and economic privileges of its governing bourgeoisie. Sternhell denies that fascism was an opportunistic defense of this bourgeoisie, even though he cites Mussolini: "The State . . . must renounce all forms of economic intervention." "We will oppose with all our strength any attempts at socialization, [economic] statism [or] collectivization" (p. 306–07).

What then did fascist "revolutionaries" retain from Marxism, if anything? What made them "socialists," however revisionist? For Sternhell, the answer lies not only in their pursuit of an "anti-bourgeois" cultural revolution (by this definition, Friedrich Nietzsche, who despised socialism, could be called a socialist) but also in their cult of violence. Revolutionary Marxism, Sternhell reiterates, also countenanced violence. But is this enough to establish a generic bond between two such

disparate ideologies as fascism and Marxism, especially when they were such fierce enemies throughout the 1920s, 1930s, and 1940s? Why identify violence only with the Left, excluding Nietzsche, Otto von Bismarck, Adolphe Thiers, Colonel de La Rocque, and other rightists? Why confine cultural criticisms of Enlightenment rationalism and Marxist materialism to the Left, when it was such a dominant feature of the political thought of André Tardieu and a host of other antipositivist conservatives? And all fascists were not as futuristic as Marinetti; most fascist ideologues during the interwar period (from Mussolini and Hitler to Georges Valois and Jacques Doriot) preached cultural traditionalism, not cultural revolution, at their mass meetings.

Sternhell does acknowledge that, although revolutionary syndicalists such as Sorel may have objected to "shameful compromises with the bourgeois right" (p. 21) and despised the "capitulation" of the Social Democrats to bourgeois liberalism, Mussolini, when he actually came to power, "compromised with existing social forces" to the point that he produced a regime "in which all the elements of socialist origin were banished" (p. 312). Apparently, fascist ideology was not as "autonomous" (p. 12) as Sternhell at first suggests.

Although one may dispute Sternhell's formula that fascism originated primarily in a revision of Marxism rather than conservatism, the great value of his work is his account of how some of Europe's most brilliant intellectuals during the interwar period were attracted to fascism's glorification of irrationalism. "When *anti-rationalism*," he writes, "becomes a political instrument, a means of mobilizing the masses . . . against liberalism, Marxism and democracy, when antirationalism is associated with an intense cultural pessimism, when it is coupled with a pronounced cult of violence and activist elites, then fascist thought fatally comes into being" (p. 340).

ROBERT SOUCY
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ROBERT H. WHEALEY. *Hitler and Spain: The Nazi Role in the Spanish Civil War, 1936–1939*. Lexington: University of Kentucky Press. 1989. Pp. ix, 269.

In the preface to his book, Robert H. Whealey poses questions about Adolf Hitler's role in Francisco Franco's rise to power and Hitler's gains, both in Spain and throughout Europe, from the Spanish Civil War. He should have focused only on Hitler's gains. Whealey is primarily an economic historian; most of his book comprises an informative essay on the relationship between Franco's Spain and Hitler's Germany from the early months of the Civil War to its last days and on the diplomatic effect on Germany. Yet he weakens his conceptual scheme of this relationship when he ventures, in several sections, into the ideological comradeship between Franco and the fascists.

Whealey describes Franco as a "counter-revolutionary . . . unlike Hitler and Mussolini" (p. 41); however,

he makes the common mistake of identifying Franco with Spanish fascism and fails to distinguish between the Falange in its true fascist phase under José Antonio Primo de Rivera and in its later altered character. He contends that Franco "moved close[r] to the fascist concepts of Hitler and Mussolini" and links him with the martyred José Antonio (p. 42), ignoring Franco's merely expedient appropriation of Spanish fascist ideology in his usurpation of the Falange organization.

Whealey states that "Franco and Hitler both won the Spanish Civil War" (p. vii). Certainly Franco won. Hitler's "victory" remains debatable, considering Whealey's contention that Hitler pushed Mussolini to take the major Axis role. Whealey also suggests that Hitler's maneuvering in Spain was primarily motivated by his desire to upset the balance of power elsewhere, hardly a way to "win" in Spain. In his delineation of the economic relationships set up by the Germans with various Spanish groups, however, Whealey does much better. This is his territory, and he clearly knows his way around the multitudinous documents available in Germany, Spain, and Italy.

Whealey attempts to establish a systematic reconstruction of the changing relationship between Germany and Spain from 1936 through 1939, starting with Hitler's adventurous transporting of Moroccan troops to Spain in the first days of the war. He recognizes Hitler's vacillation and eagerness to let Mussolini act for the Axis in providing aid to Spain, yet he emphasizes the presence of the Legion Condor throughout the war and interprets its presence as an indication of Hitler's basic commitment to Franco's cause. Whealey makes Hitler appear more systematic than he actually was in his pursuit of diplomatic gains through intervention in the Spanish war. He never comes to grips with Hitler's ambiguity and tactical flexibility—two qualities that characterized the latter's behavior especially before 1941.

This book's details and extensive notes will be useful to students of the economic relations between Germany and Spain in the period before World War II. The author tries, however, to cover too much in too little space. Although many of Whealey's conclusions are plausible, his definitions of such terms as "fascism" and "totalitarianism" not only detract from his economic and diplomatic analysis but also weaken his arguments by raising doubts about Hitler's ideological motivation.

PATRICIA ROOT FOUQUET
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DETLEF BRANDES. *Grossbritannien und seine osteuropäischen Alliierten 1939–1943: Die Regierungen Polens, der Tschechoslowakei und Jugoslawiens im Londoner Exil vom Kriegsausbruch bis zur Konferenz von Teheran.* (Veröffentlichungen des Collegium Carolinum, number 59.) Munich: R. Oldenbourg. 1988. Pp. 607. DM 178.

Those willing to plough through this densely packed book will find a rich reward. Not a conventional study

of international relations, it is mainly concerned with how the British government in the early years of World War II dealt with the governments-in-exile of Poland, Czechoslovakia, and Yugoslavia, which were resident in London and inevitably subject to British protection and influence; with the resistance movements and related political developments in those countries; and with problems of diplomacy and planning relevant to the place and role of Eastern Europe in the postwar world. In all of these respects the author has done an outstanding job of research and presentation of his material.

The bulk of the documentation comes from British records (War Cabinet, Prime Minister's Office, Foreign Office, but unfortunately not those of Special Operations Executive and other intelligence agencies), admirably supplemented by Polish, Czechoslovak, and Yugoslav published and unpublished sources. The author is able to follow not only all of the twists and turns of British policy as it was decided at the top but also the ideas, plans, discussions, and disagreements within the government, especially those of the high career officers in the Foreign Office. A reading of their memorandums and position papers gives an idea of how uncertain and at times unrealistic the British were in their planning for postwar Europe. Perhaps this was not so surprising at the stage in the war in which they fought alone for their very survival and did not focus on a new Europe that might never come into being, and perhaps the issues were increasingly irrelevant after June 22, 1941, as it became apparent that the fate of Eastern Europe would be in Russian, not British, hands.

The book is especially good in illuminating the question of federation in Eastern Europe, an idea elaborated by the Foreign Office research department, adopted by the War Cabinet, and pushed by Foreign Secretary Anthony Eden and the Foreign Office in their dealings with the governments-in-exile and with the USSR. Its supporters hoped or expected that it would improve on the failed post-World War I settlement, overcome nationality conflicts within the region, and promote economic viability, but the main rationale was that it would contribute to security. Against threats from what direction? From Germany, of course, but what about Russia? Could the Russians be co-guarantors in some Council of Europe or the like without taking charge? Would they drive the East European nations back into the arms of a revived Germany? The question for London was how to deal with them while the war was still going on, what to negotiate, and when.

Brandes explains with ample documentary evidence how Eden's school of thought carried the day. He believed it necessary to be forthcoming with Joseph Stalin, to dispel all suspicions of a *cordon sanitaire* policy, and to win his cooperation by showing understanding for Soviet interests in Eastern Europe, not just for military reasons but for a stable postwar world—this at a time when it was the Soviet Union that needed the understanding and help of the Western powers to carry on the war. The anti-appeaser on the issue of

Munich became the appeaser in dealing with Stalin. Eden gave way on a whole series of questions, including acceptance of Soviet annexation of the Baltic states, relocation of Poland's frontiers from east to west, and abandonment of the Polish-Czechoslovak and Greek-Yugoslav federations that the British had crafted with the governments-in-exile. At the Moscow conference in 1943, Eden proposed his federations in general terms and simply deferred to Viacheslav Molotov's veto. The contrast in relations with the Czechoslovak and Polish governments was very revealing. Eden was, though sometimes reluctantly, accommodating to Eduard Beneš and his policy of putting his country's fate in Soviet hands. He was tough to Władysław Sikorski and Stanisław Mikołajczyk, who were trying to preserve Poland's territory and independence by resisting the Soviets.

Strangely enough, Winston Churchill did not assert himself strongly on these issues at this time. By the time of the Tehran conference and by virtue of the decisions made there (Curzon line for Poland, no Anglo-American move into the Balkans, aid to Tito in Yugoslavia), the die was cast. Brandes calls it an Anglo-American de facto recognition of a Soviet sphere of influence in Eastern Europe. American officials certainly did not consider that to be the case. Cordell Hull and the State Department continued to inveigh against the entire concept of spheres of influence. Yet it must be said that Washington had provided no support to those in London who might have taken a stronger line and that Roosevelt's firm convictions on four major points—primacy of military decisions, postponement of all political and territorial issues until the peace settlement, hope for a new world security system, and confidence in his ability to establish cooperative relations with Stalin—made any other outcome most unlikely.

Would the book's study, impressively documented on the British side, look different with comparable investigation on the Soviet or the American side? Whether there are any revelations from Soviet sources depends on the Soviet government more than on Western scholars. Steven Miner's *Between Churchill and Stalin: The Soviet Union, Great Britain, and the Origins of the Grand Alliance* (1988), which was published after Brandes completed his manuscript, has some interesting comments on Soviet and on British policy in the 1940–42 period and may be read profitably in conjunction with the Brandes book. As for the American side, this is not unknown terrain, although Brandes limits himself by not going into the diplomatic archives beyond what is published in *Foreign Relations of the United States*. One final point: in a massive volume that time and again goes into detail on complex territorial questions, there is not one map.

JOHN C. CAMPBELL
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ERIC KERRIDGE. *Trade and Banking in Early Modern England*. Manchester: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. 185. \$39.95.

Banking through banks and banking outside of banking institutions are two distinct developments, which have been confused by another practice, namely, the use of banking-like instruments among merchants. After banks came to dominate the traffic in negotiable instruments in England during the nineteenth century, historians of banking came to assume that trade in financial instruments such as bills of exchange, promissory notes, and bills on London implied banking proper. Merchants dabbling in notes seemed such close cousins of bankers that important distinctions were lost, to the discredit of the historical role of bankers and their relation to trade.

In this brief book Eric Kerridge demonstrates certain connections between trade and banking to prove that the origins of English banking lie in mercantile roots. He is more convincing in showing that merchants did their own "banking," that is, that they used certain banking-like instruments and functions that later became the domain of banking. With few exceptions—the Gurneys of Norwich being one—the clients of bankers before 1750 seem to have been landowners rather than merchants. Although the agrarian economy rested on trading markets controlled by merchants, that connection was an indirect link with banking that answers few specific questions relevant to the history of early modern English banking, notably, where the line between brokerage and the use of money begins. There is logic in Kerridge's argument that the rise of metropolitan markets in the sixteenth and seventeenth centuries necessarily promoted the high finance associated with banking. But the facts suggest that it was the disruption of local markets from 1642 to 1660 that ended the local credit system that royalist landowners needed to pay composition fines. This increased their dependence on the banking of the goldsmiths and scriveners of London, thereby specializing the process.

Kerridge's examinations of the theory and practice of the bill of exchange and the bill on London are excellent and fortified with much new manuscript material. But these could have been published as articles apart from a book. The work ends with a discussion of the price revolution, without a conclusion as to how this complements the author's general theory. The imprecise chronology of this book and the sweeping generalizations argued from weak, local evidence (for example, pp. 77, 98–99) often ignore the rapid changes in banking history and the differences in banking operations. We are told that the Bank of England was "a scaled-up version of the goldsmith-banks... its foundations were set on the rock of commerce, a discounting bills of exchange, and above all inland bills" (p. 81). This distorts the role of both public and private banking, especially after the early eighteenth century when the bank's monopoly of joint-

stock partnerships restricted private banks to an inferior but different competition. Perhaps Kerridge should have made another case, that is, that merchants throughout the period had little direct need of banks to execute their operations and that it was the sophisticated arrangements merchants made among themselves that delayed the union of trade and banking for so long.

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ALICE T. FRIEDMAN. *House and Household in Elizabethan England: Wollaton Hall and the Willoughby Family*. Chicago: University of Chicago Press. 1989. Pp. xvi, 227. \$39.95.

This book is an example of a relatively new genre, that is, the social history of architecture or, more popularly, the biography of buildings. The principal inspiration for this sort of writing derives from Mark Girouard's *Life in the English Country House* (1978). Alice T. Friedman teaches art and architecture but is knowledgeable about Elizabethan social history as well; she has drawn materials from several British archival collections together with published works.

Wollaton Hall was one of the greatest country houses of the later Tudor period, in the same league as Longleat and Hardwick. Its owner was Sir Francis Willoughby, its architect Robert Smythson. In style the building was boldly experimental. It included classical features mediated through the Italian villa and the French chateau. Its plan was extraordinary in several ways. Instead of clustering the rooms around an open central court, Smythson placed the great hall in the middle of the house, lighting it with clerestory windows. Above the hall he built the "prospect room," a special chamber giving fine views of Willoughby's estate. Two parallel suites of ceremonial rooms enveloped the hall. These were not, as in some other houses, sets of rooms for the husband and wife; instead, both were male-dominated, one for Willoughby's use and one for visiting dignitaries.

The history of the Willoughby family, aptly described by Friedman, is melancholy. Sir Francis and his wife suffered continual marital difficulties. She left him and lived in London while the great house was being constructed. Even when she returned, she refused to live in the new building, so Wollaton was used for entertaining, as a showy satellite, while the family continued to occupy an older house nearby. Lady Willoughby was chronically ill, and she and her husband had no son, although they did have six daughters. After her death Sir Francis married a much younger woman, who was suspected of taking his money and poisoning him. The succession was disputed; the estate was encumbered by debt; Wollaton remained vacant for some years; and the second wife removed its original furnishings. The present state of the house is

not ideal either. It is owned by the University of Nottingham, which is using it as a museum of natural history rather than filling it with appropriate furniture and paintings.

Because Friedman blends descriptions of architectural style and planning with accounts of family troubles, her book should be of interest to both architectural historians and social historians, including those who study women's history. The volume is illustrated with more than one hundred photographs, all well chosen but some of poor quality.

STANFORD E. LEHMBERG
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WHITNEY R. D. JONES. *William Turner: Tudor Naturalist, Physician, and Divine*. New York: Routledge. 1988. Pp. 223. \$42.50.

The multifaceted career of William Turner must have put off many biographers fearful of treading unknown turf. The religious historian is seldom at home with the herbalism and ornithology that occupied so much of Turner's attention, and it is a rare historian of medicine who is willing to take on the virulent religious polemic that was no less important to the subject of this biography. Whitney R. D. Jones has managed by treating Turner successively as naturalist, physician, and religious reformer in order to shed some new light on both the history of the English Reformation and the breadth of interests that seems to have characterized the Renaissance scholar. Where this successive treatment fails is in suggesting any integration whatsoever of these disparate interests of Turner. There is no attempt to suggest how Turner's activities or ideas in one area may have affected his approach to other areas.

It is evident from Jones's account that Turner was not a scientist in the modern sense. He was an observer and a recorder of data, publishing descriptions of plants, birds, and fish, tedious to the nonnaturalist reader of Jones's account but undoubtedly important in laying the groundwork of English botany and ornithology. These and Turner's descriptions of diseases and prescriptions for ailments, quaint and occasionally amusing, are exemplified at excruciating length in two slow-moving chapters. Turner was an uninventive physician (for the Duke of Emden and later for Somerset's household); the primary importance of his medical studies is described here as his use of corporal imagery in discussions of religion—*de rigueur* in any case in the sixteenth century.

The two longest chapters surveying Turner's career and discussing his involvement in religious and social reform help to redeem the book and make it a useful source for students of the English Reformation and of origins of Puritanism. Turner was involved with the central figures of the early Reformation, was exiled under both Henry and Mary, and actively advocated further reform in Edward's reign and the first decade of Elizabeth's when the foundations were laid for what

must surely be called Puritanism. He was a pupil of Nicholas Ridley, converted by Hugh Latimer, and a colleague of Thomas Becon in the Protector's household. Later, his fierce antipopery, opposition to vestments, and call for congregational participation in the appointment of ministers and lay participation in the administration of church discipline would place him well within the precincts of the "hotter sort" of Protestants. As a social reformer, Turner was fundamentally conservative, evincing more old-fashioned moralism than progressive fervor for institutional change. Jones's claim of Erasmianism for him is not well founded; he is more correctly identified with the likes of Robert Crowley. Jones might have pointed out, however, that a more incisive critique of institutions and a more radical insistence on social change were available in the mid-Tudor period.

The strength of this biography does not lie in its placement of Turner in context. At a minimum, some attempt to relate him to other Elizabethan Puritan scientists (William Fulke, for instance) would be useful in continuing the debate about early modern religion and science. There are also some curious anachronisms, such as his association of Puritanism with the prudery of a much later era. Finally, on the issue of religion in Tudor Oxford and on Elizabethan Puritanism, Jones would have benefited from consideration of more recent scholarship. As a narrative and a catalogue, this is a useful work; for a compelling analysis of medicine, natural science, and religion in the sixteenth century, we will have to look elsewhere.

MARGO TODD
Vanderbilt University

CHRISTOPHER HILL. *A Tinker and a Poor Man: John Bunyan and His Church, 1628–1688*. New York: Alfred A. Knopf. 1988. Pp. xvii, 394. \$22.95.

Christopher Hill's research interests have moved to the latter part of the seventeenth century in recent years, although his focus continues to be radicalism and "the industrious sort of people" (see, for example, *Society and Puritanism in Pre-Revolutionary England* [1967], p. 124). This work was first issued by the Clarendon Press under the title *A Turbulent, Seditious, and Factioned People*.

Although Hill admits that John Bunyan was "politically unsophisticated," he attempts to read an ideology of resistance into his writings and put them into historical context. Very few facts are known about Bunyan's life, and Hill has little of a strictly biographical nature to add. Rather, the author sets about to extract all of the coded messages from Bunyan's text that one never saw when suffering through *Pilgrim's Progress* as an undergraduate. Although this makes for a long and discursive book, it is also a compelling and instructive introduction to the literature of the English Puritan and radical traditions in the second half of the seventeenth century.

It is now clear, in light of recent research, that many more radical sects continued to engage in political conspiracies during the reigns of Charles II and James II than historians used to think, and it becomes easier to understand the political motives behind Bunyan's trial and long imprisonment. *Pilgrim's Progress*, *The Holy War*, and other writings reflect the various political and religious crises of the Restoration period with its many persecutions and purges of municipal corporations (such as Bedford, where Bunyan lived in his latter days). It will always be problematical interpreting the political meaning of Bunyan's allegorical writings, but his choice of printers—up to their ears in seditious conspiracies—leaves no doubt about his radical inclinations. The author argues that Bunyan saw persecution in social as well as religious terms. Most ordinary people were more likely to experience persecution from the gentry rather than from the Anglican clergy: this took the form of the enclosure of commons and the extinction of use-rights or the enforcement of the vagrancy laws, which could imprison the humble or reduce them to servitude. Thus, the persecutions of the Restoration were directed not only against nonconformists but also against vagrants and traveling tinkers. As an itinerant preacher who refused to be silenced, Bunyan suffered on both counts. Hill insists that Bunyan's writings frequently display "class-awareness," a term that he now prefers to "class-consciousness" (p. 366). Bunyan consistently assumed that the gentry were all of Norman descent and members of the seigneurial class. Many of the evil characters in Bunyan's writings are genteel.

The dichotomy between rich and poor was, of course, never that simple, and Bunyan forgot the many small gentry and younger sons who fought in the New Model Army and made their way into the radical sects. Moreover, the extinction of common rights hurt gentlemen copyholders as well as husbandmen. But the gentry supporters of Dissent disappeared within one generation of the restoration of Charles II: they had to conform if they wished to retain a political voice and avoid being social outcasts.

Thus, the nature of Dissent promoted social polarization, and this, according to Hill, is how Bunyan developed that "class-awareness," which he finds permeating Bunyan's writings. Bunyan was scorned by learned and respectable society in the eighteenth century but became highly esteemed when Nonconformity acquired respectability in the nineteenth century. By that time Nonconformists could no longer decode the message that Bunyan's writings contained, and missionaries were horrified when they translated *Pilgrim's Progress* into Chinese and the Taiping rebels adopted it as their own.

ROGER B. MANNING
Cleveland State University

PAUL SEAWARD. *The Cavalier Parliament and the Reconstruction of the Old Regime, 1661–1667*. (Cambridge

Studies in Early Modern British History.) New York: Cambridge University Press. 1988. Pp. xii, 359. \$49.50.

Until recently the 1660s were a neglected decade in English history. There were problems with the sources, for one thing, and, for another, the subject matter was unpromising. One failure after another, at home and abroad, was the record of the central government. And for those interested in social history things were equally gloomy, for the 1660s was a decade of economic depression and perhaps even of demographic decay. Best to leave it to the readers of Milton, Marvell, Pepys, or Newton and move on to something more cheerful.

In 1985 the situation changed with the publication of Ronald Hutton's remarkable book *The Restoration*. Hutton is one of the very few people since Pepys who has been able to make the 1660s fun. His scholarship is just as good as his prose, and any pedants who may denigrate him for being readable do so at their peril. His book is a delight and will long remain one. Now we have Paul Seaward's solid monograph. Given the economic problems of recent years, it is admirable that scholarship of any kind can still be done in Britain. That makes the very high standard of the series "Cambridge Studies in Early Modern British History" all the more impressive. Seaward's book is a worthy addition to the list.

Seaward attended one of Hutton's seminars, and it may be that the two men entered into a treaty to delimit boundaries. If so, it is rather a pity; Hutton got almost all of the good stories and left Seaward with a book that cannot quite stand by itself. It is parliamentary history of the most austere variety, arranged topically, with few concessions to human weakness. One does not ask for a description of Lady Castlemaine's chamber pot, if only because Hutton has given us that. But Seaward could have made more mention of the physical problems of Clarendon and Southampton. Were they too ill to function? Most of us think that they were, from time to time, and that their illnesses had a real impact on the course of parliamentary history.

Parliamentary history by its nature makes things sound more pugnacious, more combative, than perhaps they really were. And it must also give greater prominence to the House of Commons than that body perhaps deserves. For the most part, Seaward and Hutton seem to be in broad agreement, though there are significant differences, too. Seaward gives less emphasis to Arlington than he might, and he stresses the nuisance caused by Archbishop Sheldon and by the Irish Cattle Bill. His contribution is precisely that he has given us such a detailed and thorough history of the first years of the Cavalier Parliament. What we need now is a good book on Arlington.

STEPHEN B. BAXTER
University of North Carolina,
Chapel Hill

D. W. JONES. *War and Economy in the Age of William III and Marlborough*. New York: Basil Blackwell. 1988. Pp. xviii, 351. \$60.00.

D. W. Jones sets a dual purpose in this excellent book, first to show the effect of the economy on the progress of the War of the League of Augsburg and the War of the Spanish Succession and, second, to show the effect of the wars on the economy of England during the same period. Today military historians believe wars were won and lost in the counting houses as much as in the battlefields, and Jones's book is a contribution to that effort. Each war happened in a different economic climate. The severe economic distress of the 1690s caused critical problems in meeting the expenses for material on the continent. With spending and consumption happening abroad instead of at home, England risked a serious deficit of spending and a fall in domestic output and employment. Jones shows in detail how the crisis of meeting the gold shortage was solved by coin clipping and then recoinage as well as by the vagaries in trade that created a slight balance. At the same time, war on the continent ruined a number of industries and markets where England reaped record sales of grain and woolen textiles after 1700. This, together with the discovery of gold in Brazil and the union of the two East India companies, meant that the Succession war was fought in a more favorable economic climate, even though the losses through war and trade were severe.

The wars had the ultimate effect of stimulating growth in the English economy after 1700, when the bulk of all spending was directed to supporting soldiers and horses rather than to the industries associated with equipment and weaponry. Civilian demands for iron and metal goods were great enough to compensate for losses suffered during the slumps in the wars, and this expansion of domestic industries during the wars increased greatly in the Spanish and Portuguese colonial markets both before and after the end of the Succession war. The trade surpluses were directed into internal development, including agriculture. Jones presents an impressive list of merchants and financiers who made large profits from the wars and who purchased considerable estates during that period.

Based upon a wide range of sources, including exhaustive forays into the London port books, this study is a major contribution to economic history. The author presents elaborate proof for his argument through tables valuable to other historians working in social and economic history. His command of basic economics and economic theory is excellent, and his matchless expertise in the sources of his book represents years of hard work in archives. The structure he defines should inform the work of scholars working in diverse fields of the period.

FRANK MELTON
University of North Carolina,
Greensboro

FRANK MCLYNN. *Charles Edward Stuart: A Tragedy in Many Acts*. New York: Routledge. 1988. Pp. xii, 640. \$29.95.

Although the adventures of Bonnie Prince Charlie have been the subject of countless popular works, several of which appeared in 1988 to commemorate the two hundredth anniversary of his death, this volume is the first full-scale biography of the Young Pretender by a professional historian. It makes use of a remarkable array of sources, from the Stuart Papers to the *Archivio Segreto Vaticano* and the French *Archives Étrangères*, and it is the only life of the prince to take account of the recent upsurge of interest in the political and social impact of Jacobitism. This big, ambitious book aims at combining political, diplomatic, and military history with a psychoanalytical approach to Charles Edward's complex personality. It is not always successful, but it easily supersedes previous attempts to interpret the prince's strange career.

The strongest sections of the book deal with the genesis and course of the ill-fated rebellion of 1745, on which most of Frank McLynn's previous work has concentrated. He stresses the opportunity lost by France through its failure to support the rising, which came very close to success, and he argues persuasively that the rebel army's retreats—first from Derby, then from the Lowlands—were strategic mistakes. In general, McLynn defends Charles Edward's military acumen, arguing that his impulsiveness was more suited to the situation than Lord George Murray's plodding style. The fast-paced account of Charles's escape after Culloden makes good reading even for those who already know the story, and it effectively resurrects the traditional image of "Butcher" Cumberland. By representing the rebellion as the last gasp of a doomed clan society, however, McLynn casts doubt on his own assertion that it was not a hopeless gamble. His views would have been better served had he challenged the assumption that the Jacobite clans were economically "backward." Far from destroying the old Highland social order, moreover, the rebellion of 1745 may have helped to preserve its vestiges by fostering a nationalist myth that made the clans symbols of Scottish identity.

After the rebellion, Charles Edward enjoyed an immense international reputation. Lionized by Montesquieu, Diderot, Voltaire, and Helvétius, he quickly adopted an "enlightened" skepticism toward religion and, ironically, monarchy. McLynn chronicles these facts in detail, but it remains for some future scholar to place them within the context of Enlightenment political thought and an incipient romanticism. Mirroring its subject's life, McLynn's narrative begins to unravel after the prince's expulsion from France in 1748. The chapter on the murky Elibank plot of 1752–54 suffers from the sketchy understanding of English politics that occasionally mars the book. The dreary alcoholism of Charles Edward's later years is dealt with compassionately, but the chapters on the period after 1754 add little to what is already known, except for some intriguing pages on Vatican politics and on negotiations with

the Freemasons. The problem of how Jacobitism died is barely addressed. Did it simply drown in the prince's liquor?

Some historians will be irritated by the author's efforts at psychoanalysis. To McLynn's credit, however, he keeps these remarks brief, and most of his conclusions are well founded. Charles Edward's familial and sexual relationships were invariably disastrous and invite a psychological explanation. Once or twice, the author goes too far in his assumptions, notably in his treatment of the causes of Henry Stuart's apparent homosexuality, but he is undoubtedly correct in emphasizing that the dominant factor in the prince's mental makeup was his resentment of a domineering and overprotective father. In this as in other matters, McLynn tends to show an excessive sympathy for Charles Edward, who surely was responsible for his own failures. Nonetheless, it is refreshing to find, after two centuries of gushing praise or sneering opprobrium, that the prince's good qualities may be justly remembered and his historical importance rightly restored.

PAUL MONOD
Middlebury College

PETER VIRGIN. *The Church in an Age of Negligence: Ecclesiastical Structure and Problems of Church Reform, 1700–1840*. Cambridge: James Clark. 1989. Pp. vi, 317. £25.00.

Peter Virgin's important book compares favorably in scope and detail with Norman Sykes's hitherto unsurpassed *Church and State in England in the XVIIIth Century* (1935). Virgin has accomplished for the lower clergy and the parochial structure of late Georgian England what Sykes did for the earlier eighteenth century, the bishops, and diocesan administration, but Virgin's study, unlike Sykes's, is erected on a firm foundation of statistical evidence. Sykes knew and used the massive *Report of the Ecclesiastical Revenues Commission* of 1835, but neither Sykes nor G. F. A. Best in *Temporal Pillars* (1964) collated the raw data effectively for the period leading up to reform. Virgin's study is the first to examine systematically the clergy's wealth, their patronage, pluralism, and nonresidence. He illuminates the pattern of clerical careers, beginning with education and extending through ordination, curacy, and preferment. Insights into their careers provide the backdrop for explaining the difficulties faced by reformers. The splendid clarity of the book renders the convoluted ecclesiastical machinery of the Georgian church intelligible, and one finds a judicious balance between broad surveys of data that bear on the nation at large and helpful analyses of regional variation.

The clergy were neither as aristocratic in social origin nor as wealthy in personal income as the radicals of the age of reform maintained, nor were they as venal. Only a fifth of all of the incumbents were related by blood or marriage to the peerage, and Virgin judges this to be a

small proportion. The clergy's income came from land, and this tied them to the rural food-producing economy. Because farmers generally benefited from the enclosure movement and the economic conditions of the Napoleonic war years, so did the clergy. But Virgin shows conclusively that, in light of the beneficed clergy's social and family obligations, the average clerical income was clearly within the bounds of reason. The lot of the curates, however, and the poorer beneficed clergy was much worse than what Sykes calculated. Nonetheless, reform resulted in a reduction of the number of poorest livings, and, although the gulf between rich clergy and poor was great, Virgin shows that it was not significantly widening.

The book gently but persuasively makes adjustments in a number of Sykes's more important conclusions. Sykes had emphasized the bishops' conscientious performance of duty, but Virgin shows that bishops with livings in their gift were as prone to present them to pluralists as were wealthy lay patrons, a fact that helps account for the tardiness of reform. The growth of the extent of pluralities is more serious than historians had believed, but the number of pluralists was fewer than opponents claimed, and Virgin demolishes the argument that clerical poverty was the main motive for pluralism. The causes are many and complex, but they can be traced in part to nepotism and the rising social ambitions of the clergy. Despite all of the resistance to change, reform legislation had a measurable impact; in the two decades following 1830, for example, 25 percent more clergymen could be found in residence.

Arguably, however, reform came too late. Long before the 1830s, the church experienced a serious loss of popular support with attendance falling off dramatically to a third of the overall population in some rural areas and church seating available for only about a fourth of the people in the urban settings. Virgin's study, like that of J. C. D. Clark, is the study of an elite, and neither scholar has adequately addressed the anomaly of the clergy's enhanced status relative to the church's diminished standing in the eyes of the English people. Deryck Lovegrove's recent work on the rapid rise of Nonconformity, *Established Church, Sectarian People* (1988), suggests the need for much further research on the connections between the loss of attendance, the growth of secularism, and the purported hegemony of Anglicanism. Nevertheless, Virgin's book corrects previous accounts at significant points and provides us with the best general survey of the Georgian clergy and their efforts at reform.

JAMES E. BRADLEY
Fuller Theological Seminary

MALCOLM CHASE. *"The People's Farm": English Radical Agrarianism, 1775-1840*. New York: Clarendon Press of Oxford University Press. 1988. Pp. vi, 221. \$55.00.

The agrarian element in nineteenth-century British radical thought is frequently played down by histori-

ans. Advocates of agrarian reform are relegated to the penumbra of nostalgia or characterized as bearers of antediluvian attitudes toward industry. In particular, the disciples of Thomas Spence, a leading proponent of agrarian reform, are shunted to the margins of history. Spence's theories are assigned a minor role in the evolution of socialism; his "Spencean" followers are often condemned as "wild revolutionaries" and bogeymen of disaffection.

It is a virtue of Malcolm Chase's interesting study that it confronts these interpretations directly and inflicts fatal blows on some of them. Chase maintains that agrarianism was a rational component of popular radical thought. It offered a means of resisting the "imposition of increasingly capitalistic work-forms" (p. 6) and affirming an inclusive concept of justice against privilege and power. As Chase makes clear, specific blueprints for agrarian reform differed widely: from the communitarianism of a minor radical propagandist such as Thomas Preston, to the small-scale capitalism favored by the popular journalist William Cobbett, to the "mixed" program of the Chartist leader Feargus O'Connor, who attempted to resettle workingmen on the land as self-sufficient farmers. What these agrarian radicals had in common was a belief in the land as a lever against distortions of industry and commerce. They (and other popular radicals of varied political hues) used rural ideas and imagery to combat unwelcome aspects of change and to pursue their agendas for improvement. In so doing they drew on clusters of feeling whose roots were nourished in the Enlightenment and in religious experience as well as in customary attitudes to work and leisure.

Unfortunately, this book, although a helpful corrective to several outmoded interpretations of the period, promises more than it fulfills. It veers uneasily between rigorous intellectual analysis and a detailed account of underground radicalism of the kind purveyed with greater success by E. P. Thompson and several other historians. Chase assigns more weight to Spenceanism as a shaping force in early nineteenth-century thought than it can bear (the book is essentially a narrative account of Spence and his followers); he also pays too little attention to the influential cluster of social and cultural attitudes about the land that permeated popular thought. Had Chase decided to focus on this broader "pastoralism" in his book as, for example, Martin Wiener does in *English Culture and the Decline of the Industrial Spirit, 1850-1980* (1981), he would almost certainly have written a definitive study instead of a merely useful one.

JOEL H. WIENER
City College,
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MARK HARRISON. *Crowds and History: Mass Phenomena in English Towns, 1790-1835*. (Past and Present Publications.) New York: Cambridge University Press. 1988. Pp. xviii, 355. \$49.50.

The "crowd" holds a special place in history, both as it is enacted and written. The central emphasis of Mark Harrison's argument is that the received tradition of crowd histories involves a selective inattention that works against a proper understanding of mass phenomena during the urban and industrial revolutions of England. The actual focus of study has been "disorder" and "riot," thus excluding much more common instances of crowd behavior—processions, celebrations, ceremonials, election meetings, and other forms of recreational or formal gatherings—that were usually peaceful and orderly. What Harrison sets out to do through a detailed analysis of crowds in Bristol, together with some comparative materials from Norwich, Liverpool, and Manchester, is not only to redress the balance but also to understand how the equation of "crowd" and "riot" was fashioned through the interaction between political elites and crowds in this transforming period of English history.

What Harrison places on offer is, therefore, much more than just another crowd history. The extent to which he succeeds in his highly ambitious project is, however, somewhat uneven. Once he sketches the field of vision, chapters on the timing of crowds, their location and symbolism, and the language of crowd description are used as forays into different aspects of the ordering and perceptions of the urban environment. The argument is then fleshed out with detailed case studies of meetings and elections, celebrations and ceremonials, and finally the Bristol Bridge riot of 1793 and the notorious "reform" riot of 1831.

By unraveling the complex and overlapping meanings of crowd events, Harrison uses crowd phenomena as a prism through which to diffract the rhythms of urban life. The discussion of the specific circumstances under which crowds were labeled as "mobs" provides a telling illustration of how political elites attempted to manipulate the image of the crowd. Large assemblies of people could inspire both awe and delight, as well as fear and anxiety, and could be used to signify either civic pride or symptoms of demoralization. The discussion of the timing of crowds, however, on which Harrison tries to hang a weighty argument on the orderliness of towns, is not entirely convincing and involves sometimes slim and contentious evidence. He is also probably unwise to claim a comparative status for his study, given that the bulk of his research focuses on Bristol and that he offers only the thinnest of detail on Norwich, Liverpool, and Manchester. It is a matter of some regret that Harrison did not cite John Bohstedt's *Riots and Community Politics in England and Wales, 1789–1810* (1983), which could have strengthened the comparative grasp.

This book is nevertheless useful and interesting, and Harrison is surely right to broaden our understanding of the "crowd." A criticism often leveled against crowd historians such as George Rudé, Eric Hobsbawm, and E. P. Thompson is that by necessity they have relied to a large degree on criminal proceedings brought against those caught up in crowd disorders and riots. A serious

methodological difficulty for Harrison is that no prisoners are taken from orderly assemblies, so that, even allowing for the problems in translating arrests and convictions into a secure understanding of the membership and motives of a riot, orderly crowds leave even fewer traces. His means of circumventing this difficulty, through a detailed attention to location and symbolism, is highly inventive. There is nevertheless one difficulty that he cannot overcome, which is reflected in the highly enjoyable narratives of the riots of 1793 and 1831 that provide the conclusion to his study. When all is said and done, riots are simply more interesting.

GEOFFREY PEARSON
Goldsmith's College,
University of London

ERIC HOPKINS. *Birmingham: The First Manufacturing Town in the World, 1760–1840*. London: Weidenfeld and Nicolson. 1989. Pp. xiii, 222. £20.00

Eric Hopkins's book is not the conventional urban history that one might easily assume from the title. There is virtually nothing in here, for example, on town politics—even Chartism is excluded—nor much on the growth of the city. There is very little detailed analysis of neighborhoods, the growth of urban institutions, or amenities. Apart from a few of the large industrialists such as the ever-present Matthew Boulton and the buttonmaker John Taylor, discussion of the town's elite is confined to interesting individual biographical sketches.

Hopkins's purpose is to suggest that the Birmingham experience during the Industrial Revolution was typical of the processes of economic and social growth and that the "shock cities" of Manchester and other cotton factory towns were very much the exception. It is certainly true that economic and industrial expansion in Birmingham occurred within a "traditional" context. The division of labor, already the key to Birmingham's success, was intensified; the units of production tended to remain small, although their numbers increased; and well-established technology that was ancillary to hard labor was modified rather than displaced by labor-saving machines. As Hopkins points out, although the modern steam engine was first produced in Birmingham, it was little used there and instead employed in the Cornish mines and in the Lancashire factories.

Birmingham possessed a well-established industrial, commercial, and distributive base long before the Industrial Revolution. In this respect it reflected the wider dynamism of British society as a whole. Thus, like the metropolitan economy, it was well placed to supply the expanding domestic and foreign markets for small metals and other goods during the eighteenth century. The best part of Hopkins's book is the core section on the economy, whose themes (and sometimes examples) tend to get repeated in the remaining pages.

Hopkins's emphasis on the survival of traditional economic forms and structures of social relations is now virtually commonplace, and it is not clear exactly what contribution to that debate this book is meant to make. There is nothing here that strikes one as really new; even the details of the small workshop gun, button, and brass trades are familiar from the work of others. The argument about Birmingham's typicality is a natural corollary of the revisionist interpretation of the Industrial Revolution but, like this book, is of limited interest.

RICHARD PRICE
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College Park

A. ROGER EKIRCH. *Bound for America: The Transportation of British Convicts to the Colonies, 1718–1775*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xvi, 277. \$45.00.

The Transportation Act of 1718 heralded a new age in British criminal justice: the introduction of a systematic punishment for capital convictions that would enhance social peace without endangering traditional freedoms or impinging on the public purse. In the long run, stories of convicts perishing at sea by "going to Hell in a Cradle" (p. 86) facilitated the development of hard labor services and the growth of the prison and the reformatory. A. Roger Ekirch uses the *Old Bailey Sessions Papers*, the *Accounts of the Ordinary of Newgate*, and a wide range of specialized literature to examine in part 1 of his book ("Justice") the crime, criminal law, prosecutions, and society, especially in London and the Home counties, that spawned the criminal exile; in part 2 ("Penance"), he treats the history of the exile in the American colonies, particularly in the Chesapeake provinces. The result is an erudite, engaging, and well-written study that will serve this area of inquiry for a considerable time to come.

On the British background, there is a shrewd assessment of judicial discretion, the role of juries, influence peddling, the pardoning process, and community concerns. The contract system spelled out in chapter 3, together with the problems of implementing the transition, is admirably illuminated. And, on the human side, the fears of the convicts are portrayed with insight. More problematic is the author's attempt to discuss the very inconsistent, quantified data on transportation (table 1) and his reliance on evidence from London and the Home counties that still dominates the historiography of the subject. Whether there was indeed a rising crime rate in the first half of the eighteenth century is questionable given the recent Gurr-Stone-Sharp debate. But the author's ability to draw on some useful evidence from Ireland and Scotland reflects his goal of striving for balanced scholarship in the midst of a polemical minefield.

The evidence of transported convicts in the American colonies provides some interesting conclusions.

Convicts were both functionally useful and economically profitable. They were less costly than slaves, and their agricultural and craft work experience proved fruitful for employers. In return, they were harshly treated, had little family life and few bonds of support, and did not acquire the motivation to carve out a future in the New World. More startling are the conclusions that British exiles committed fewer criminal acts per capita (under 7 percent) than the franchised population and had a low rate of escape (under 9 percent), and most exiles who returned to Britain resumed their former careers. Ekirch's work in the court records (pp. 171–77) is a model of analysis. There are, however, a few unanswered questions. Why so many should have remained in the Chesapeake area (97 percent) is still unclear; why so few resorted to crime in the colonies is not adequately explained; and why they did not find the burgeoning cities of the Eastern Seaboard havens for later careers is still intriguing. All good books, however, leave the reader with important questions, and excellent scholarship, clear writing, good editing, and a genuine index make this volume one to read again.

LOUIS A. KNAFLA
University of Calgary

COLIN HOLMES. *John Bull's Island: Immigration and British Society, 1871–1971*. London: Macmillan. 1988. Pp. x, 448. Cloth £33.00, paper £10.95.

In the old days many of us were taught that the conquest of 1066 had completed the national "blend" in England, once Roman invaders had been succeeded by Angles, Saxons, and Jutes, Vikings and Normans. That there was in the modern period substantial "emigration to the colonies" one was also given to understand. That there was at the same time a reverse flow—parallel and about equal in magnitude—of immigrants who left their impress on the national life was largely left unsaid. Britain is "an immigration country," and the British are "among the most ethnically composite of the Europeans" (pp. 276, 3). Colin Holmes's book registers this fact and points up something of a historiographical revolution of which he has been one of the pioneers.

For writers or revisers of textbooks and course syllabi who are not content to make passing reference to the Irish, Jews, and perhaps Pakistanis and West Indians, this book is a great boon. The discussion of many immigrant groups—about half of the forty-five nationalities represented among the children lining up before a London school in a well-known photograph of the 1960s—enables the reader to sort out parallels and differences among them. With an eye to complexity and warnings against "the dangers of simplicity" (p. 315), Holmes sets forth who came, why, and in what numbers; what their social and political lives were once they arrived; and what reactions they elicited from successive British governments and from the popula-

tion at large. In this last category, he traces the complex evolution from the Victorian absence of restrictions on foreigners, which seems as remote as the Stone Age, through the progressively harsher immigration acts of 1905, 1914, 1919, and 1962 and the epochal one of 1971, a monument to Powellism, which preserved free entry only for Commonwealth citizens with "patrial" links to Britain, "patrial" being a circumlocution for white.

The book is an inquest on the traditional belief that Britain has always been open to refugees and immigrants of every kind and persuasion, all of whom are freely accepted and wonderfully tolerated. That ancient Anglophile assumption has to be much qualified. Violence against aliens, from the attacks on Chinese in 1911 to the more recent "Paki-bashing," is as native a product as Yorkshire pudding. Holmes depicts "a wide range of hostile responses . . . from subtle to crude, overt to discreet, in both thought and action" (p. 295). The enmity, like the tolerance that prevailed in other instances, is a very complex phenomenon and stems from the interaction of many factors, no one of which by itself is necessarily determinative: stereotypes, numbers, wartime phobias, economic conditions, perception of a group as unassimilable or as threatening competition for jobs, housing, and so forth. And "racism," unless the term is defined and used with "a full sensitivity to a particular historical context," does more to obscure than to illuminate (p. 303).

The book would have been enhanced by some statistical charts; the select bibliography is too select, forcing one to trail through many citations of "op. cit." in the endnotes, which are, otherwise, an excellent guide to research; an epilogue would have been welcome to summarize matters after 1971 (as a few pages of the introduction deftly do for immigration to 1871). But this is a splendid book to which one cannot do justice in a short review.

FREDERICK M. SCHWEITZER
Manhattan College

DAVID MAYALL. *Gypsy-Travellers in Nineteenth-Century Society*. New York: Cambridge University Press. 1988. Pp. x, 261. \$49.50.

The title of this book might suggest a trivial account of an exotic subject—the swarthy Romany with his horse-drawn caravan and romantic nomadic lifestyle—but the book offers more historical substance than that. David Mayall examines Gypsies as a group and societal reactions to their nomadism and unrespectable way of life in nineteenth- and early twentieth-century Britain. He uses the term "Gypsy-travellers" to describe the group he studies, people who traveled as a way of life for much of the year. He distinguishes them from both the tramping artisans, navvies, and agricultural laborers who traveled to find work and the narrow racial image of "true Gypsies" as constituting a separate Romany race. Some of the most interesting parts of the

book concern the images and stereotypes of Gypsies propagated in the nineteenth century, and Mayall effectively demolishes the notion of "Gypsy" as a separate racial category.

The first part of the book discusses the numbers and distribution of Gypsies in nineteenth-century Britain, the nature of their itinerant life, and the sorts of work that they did to support themselves. Part 2 concerns the images of the Gypsies, with a good discussion of definitions and racial stereotypes. Part 3 deals with societal responses to Gypsies and the problems they posed for respectable Victorian society with their refusal to settle in one place and work at steady jobs as "normal" people were supposed to do. The question of Gypsy children and their education was one raised with increasing frequency and concern in the later decades of the nineteenth century. Mayall examines the Evangelical missions to convert and educate the Gypsies and the attempts, in the late nineteenth century, to use national legislation (which failed to pass) and local government bylaws (which had considerable success) to coerce the Gypsies into conforming to respectable norms. He also discusses the use of police and laws, such as the Vagrancy Act, against the Gypsies. This potentially important section is, however, rather weak in the range of primary and secondary material used and in its analysis. But the book as a whole works well; this case study of a marginal group, whose way of life led them to become nuisances in the eyes of the authorities and objects of suspicion to the general population, raises general insights into the position of an outcast minority group within an industrializing society and a growing bureaucratic state.

DAVID PHILIPS
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J. W. BURROW. *Whigs and Liberals: Continuity and Change in English Political Thought; The Carlyle Lectures, 1985*. Oxford: Clarendon Press. 1988. Pp. xi, 159. \$39.95.

J. W. Burrow has written a slim volume on a massive and complex subject. His aim is to trace the mutations in the Whig-Liberal intellectual tradition from the mid-eighteenth to the later nineteenth century. In doing so, he denies that the political culture of nineteenth-century Britain was overwhelmingly concerned with the liberal individualism usually associated with political economy, Benthamite utilitarianism, and Social Darwinism on the one hand or with the challenges to it posed by Tory paternalism and socialism on the other. Instead, he seeks to relate the central ideas of Whig-Liberalism to the problems that preoccupied so many of the eighteenth-century thinkers who developed the civic humanist tradition and the essential notions of the Scottish Enlightenment. He endeavors to show that the political ideas of Thomas Macaulay, John Stuart Mill, Walter Bagehot, and a host of other nineteenth-century Liberals were related to, or even derived from, the discussions of such eighteenth-cen-

ture predecessors as David Hume, Adam Smith, and Edmund Burke. He claims that these thinkers took up many of the same themes and showed many of the same preoccupations, although changing circumstances forced them to adjust their perspective and alter some of their conclusions. In both centuries, he maintains, many leading thinkers were concerned with the conflicts between the importance of representing interests and the challenge posed by the growth of democracy and between the fear of corruption and the hope of future progress.

In his attempt to explain the nature of Whig-Liberalism, Burrow has little say about the contribution made by political economy, Benthamite utilitarianism, or the morality of liberal Dissent. His preoccupation is with those problems that he regards as being central to the Whig-Liberal political culture. He sees the first of these as the discussion in the eighteenth century of the development of civil society from rudeness to refinement, which was followed in the nineteenth century by the anxious debate on progress. He then turns to the debate by thinkers of both centuries on the role and significance of public opinion in politics. This debate leads him to an examination of the successive discussions on autonomy and self-realization, from the eighteenth-century preoccupation with "independence" to the nineteenth-century concern with "individuality." These concepts were seen as forces capable of creating either balance or diversity, and this discussion enables the author to look at ways in which the rights of the individual could be seen as threatening to corrupt or destabilize civil society or even as a dynamic, energizing force within it. In his conclusion Burrow examines the subsequent debate on the contest between collectivism and individualism as men of Liberal views discussed the scope and powers of the state and the rights and privileges of important minority groups in the pluralistic society of late nineteenth- and early twentieth-century Britain.

This slim volume is full of perceptive comments and replete with shrewd and subtle arguments. It should interest any student of modern English political thought. Its only weakness is that the author lacks the space to develop his thesis more fully or to sustain his larger claims with a greater range of evidence.

H. T. DICKINSON
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ROY MACLEOD, editor. *Government and Expertise: Specialists, Administrators, and Professionals, 1860-1919*. New York: Cambridge University Press. 1988. Pp. xv, 359. \$59.50.

This volume breathes new life into the notion of a "Victorian revolution in government," which, after twenty years of vigorous debate, appeared to reach a synthesis in the 1970s. The studies in this collection pay tribute to the pioneering work of Oliver MacDonagh, who also contributes the concluding essay. MacDonagh

coined the expression "revolution in government" and produced a model of government growth based on pragmatic responses by officials and ministers rather than the following of ideological blueprints. The earlier debate centered on MacDonagh's provocative thesis. The essays in this volume, though by no means ignoring MacDonagh's model, emphasize another of his contributions, that is, the growing importance of the "expert" in modern British government.

Roy MacLeod's introduction frames the issues nicely and provides a well-informed discussion of the earlier debate and the ways in which these essays open up fresh fields and methods of exploration. Gavin Drewry shows the crucial importance of lawyers, because most government intervention had to emanate from well-drafted statutes. By 1857 departmental legal staffs provided four-fifths of a session's acts. But this increase in the importance of the specialist lawyer (operating under a general department head) was a corollary to the eclipsing of the "heroic" lawyer-administrator. R. A. Buchanan focuses on the burgeoning role of engineers as government experts, in spite of Samuel Smiles's image of the engineer as the hero of laissez-faire capitalism. Engineers were too important to Victorian society to be left in the private sector. Jill Pellew investigates the ways in which a changing legal and penal system prompted changes at the Home Office and required new kinds of expertise. The emergence of such new specialties as the treatment of juveniles, inebriates, and probationers eventually ended the early Victorian reliance on former military personnel. In his study of the occupational census, Edward Higgs shows the struggle over how work was classified. The earlier system used Dr. William Farr's medically oriented classification of work according to the kinds of materials employed, while the late Victorian shift (allegedly prompted by left-wing, reformist pressure) was to a system that illuminated social and economic hierarchies.

Three essays depict government experts grappling with tangible problems in the "dangerous trades" and in public health. Peter Bartrip shows how the toxic effects on workers of white lead, though recognized in the 1870s, were allowed to continue for nearly thirty years before legislation and enforcement curtailed the worst abuses. The eventual solution resulted more from the pressure of poor law boards and backbencher M.P.'s than from initiative by experts. And Christopher Hamlin depicts experts as more of a hindrance than a help in the Metropolitan Water Commissions of the 1860s and 1890s. On the other hand, Anne Hardy considers the London medical officers of health to have been highly effective, even under the direction of the much-maligned vestries, in developing preventive institutions such as the Port Sanitary Authorities.

The importance of the empire, especially Ireland, as a laboratory of governmental innovation was also pointed out by MacDonagh and is explored in a couple of essays. D. M. Schreuder demonstrates that Ireland was not only a laboratory but also an incubator of

future imperial administrators. Focusing on Hercules Robinson, he shows how Robinson became a government expert in Ireland through a process of "incremental professionalization" (p. 148) and went on to become the leading proconsul outside the Raj. John Eddy claims that the Colonial Office was the first department to experience the revolution in government, largely because of its administration of the convict system in Australia. This, he asserts, resulted from pragmatic responses to problems (*à la* MacDonagh) rather than the application of Benthamism.

The concluding essays take us well into the twentieth century. Examining the "new woman" in the administrative system, Meta Zimmich challenges MacDonagh's picture of senior civil servants as impartial, rational, and professional. Anxious to keep the civil service a gentlemen's club, they insisted on defining positions held by women as specialist (and then only in fields deemed to require "feminine" qualities), hence excluding them from the corridors of power. John Turner looks at the relationship between experts and interests, 1906–19, through the lens of Lloyd George's career. He portrays Lloyd George as having to choose between "collectivist" solutions (involving civil servants and government experts) and "corporatist" solutions (using personnel from the affected outside interests) and increasingly preferring the latter. Finally, Jose Harris returns to William Beveridge to inquire how well the MacDonagh model fits him. She declares Beveridge's career to have been decidedly idiosyncratic and to have more in common with early Victorian "heroic" civil servants than with MacDonagh's impartial administrators or twentieth-century Whitehall mandarins.

Whether they support, refine, or challenge MacDonagh's thesis, the various essays in this collection are essential reading for students of modern British administrative history and governmental reform.

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EUGENE C. BLACK. *The Social Politics of Anglo-Jewry, 1880–1920*. New York: Basil Blackwell. 1988. Pp. xi, 428. \$65.00.

This work ought to have been an important and useful study. Eugene C. Black's intentions are given in the preface: "This book examines the ways in which a highly acculturated, London-based Jewish elite developed a variety of institutions to socialize the Jewish community." He is referring, as the dates in his title indicate, to the immigration of large numbers of Yiddish-speaking Jews from Eastern Europe and the relationship to them of the Anglicized middle- and upper-class Jews of earlier migrations. Inevitably, as in American-Jewish history, that period is normally occupied with the doings of the immigrants. The established Jews have generally received a bad press; they have been accused of trying to prevent the "Russians"

and "Poles" from coming to Britain; repatriating them or sending them on to other countries; seeking to rid them of their "alien" excrescences, for example, the Yiddish language; and generally working to make them into acceptable, patriotic Englishmen and Englishwomen. This book could have been a valuable corrective to a good deal of recent Anglo-Jewish historiography, which some may think has overdone the criticism. It could also have been related to discussions in British history about questions of social control in which Gareth Stedman Jones and F. M. L. Thompson, among others, have taken part.

Alas, this is not that book. It is true that the author has used many manuscript sources as well as the contemporary press and secondary publications. He provides great detail about many individuals, institutions, and events. He examines certain features of Anglo-Jewish history that have been neglected or, at any rate, not pursued to any great extent—youth clubs and women, for example. There are many statements of fact that will be new even to specialists. Why then is the book such a disappointment? One reason must be that the author appears to have been overwhelmed by the material that he amassed. On the one hand, he seems not to have discarded anything, and it all goes in, somewhere, in the text or in the footnotes. In the process his main theme gets forgotten. Thus, when he describes the history of particular institutions, he goes into numerous arguments and maneuverings between individuals. If he was writing the history of those bodies, such detail would be central and relevant; here it merely intrudes. What is the significance of the fact that many of the organizations that were set up had financial difficulties? Did it mean that the members of the middle class were half-hearted in their social control intentions? More prosaically, the pervasiveness of such details results in the writing becoming tedious. This is only partly because, despite some new information, much of what he says is quite familiar. Yet, at the same time, parts of it have an outdated approach. To take one example, recent writings on the history of Jewish trade unionism in Britain strongly suggest that it was more important than was thought in the past. Presumably, although he has obviously read a great deal, there are lacunae in his bibliography. There is no reference, for example, to Steven Singer's work on education or to Joseph Buckman's study on Leeds (his unpublished dissertation was submitted as far back as 1968, and the book based on it came out in 1983). Black might with profit have noted some of the references in Lloyd P. Gartner's bibliographical essay "A quarter century of Anglo-Jewish historiography" (*Jewish Social Studies* 47 [1986]).

Whether or not the author had an overabundance of material to juggle, there is certainly a great deal of sloppy editing as well as poor proofreading. (Too many names, for example, are wrongly spelled.) There is too much repetition (how many times do we read that Lily Montagu did not marry Claude Montefiore?). Yet, when something is referred to more than once, the

details do not always agree. Thus, on page 17 the Jewish Working Men's Club is said (wrongly) to have been founded in 1870; on page 148 the date is given (correctly) as 1874. The first date is taken from one source, the second from another. Another example occurs on pages 128–29 concerning the number of Jewish children receiving Jewish religious education in the first decade of the century. The statistics and percentages presented there appear to be contradictory. Was it many or few?

What might have been a useful contribution to Anglo-Jewish history as well as to the debate on social control in Britain has not been realized. More care and considerable pruning might have done the trick.

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M. JEANNE PETERSON. *Family, Love, and Work in the Lives of Victorian Gentlewomen*. Bloomington: Indiana University Press. 1989. Pp. xii, 241. Cloth \$39.95, paper \$14.95.

Most writers have taken the prescriptions of Coventry Patmore and John Ruskin as accurate representations of middle-class female behavior in Victorian England and have therefore described middle-class women as playing an essentially passive role. Patrica Branca was one of the first historians of women to question this view, arguing vigorously for an interpretation that saw female members of the lower echelons of the middle class as active agents of modernization. Now M. Jeanne Peterson argues that upper-middle-class gentlewomen also played an active rather than a passive role in society. She bases her case on the records left by the women of the extensive Paget clan, a family of provincial businessmen in the early nineteenth century, whose tentacles spread into the worlds of the professions, Oxbridge, and the church in later generations. The Paget women were apparently well-educated; their relationships with men were characterized by romance, mutuality, and friendship; and their marriages were in the vast majority of cases effectively partnerships in which the wives delighted in making a substantial contribution to their husbands' work.

Such a picture serves as a useful corrective. In particular, Peterson's description of the dynamics of upper-middle-class marriage is convincing; it is likely that an idea of partnership inspired by a sense of duty as well as love provided the cement for many marriages beyond the well-known example of Sidney and Beatrice Webb. But the rosy picture she paints does beg questions. For example, although the Paget women were in many cases learned, their education was, as Peterson admits, chaotic. It is not difficult to find examples of other Victorian women, well educated by late twentieth-century standards, who nevertheless deplored the ill-disciplined and patchy nature of the

education, especially when compared to that of their brothers; the novelist Mrs. Humphry Ward would be a good case in point. The Paget women did not complain, but it is difficult to know how far it is possible to generalize on the basis of this one, albeit extensive, family. For every Paget woman encouraged to earn (Peterson finds two), it is possible to find a Victorian gentlewoman whose father expressly forbade such activity.

Peterson's interpretation of her evidence is optimistic. On the whole, the Paget women describe themselves as doing what they wished and as being content. But it should be possible to accept this without necessarily denying the existence of structural constraints—a set of boundaries—within which they moved freely. Such records as we have of late nineteenth- and early twentieth-century working-class women's lives often provide evidence of independent action, vitality, and resourcefulness. Yet the unstated constraints of poverty, ignorance, and marital dependency may also be clear to an observer. Peterson makes an important point when she argues that gentlewomen wielded an authority derived from birth and status and that this enabled them to act forcefully. But what is missing in the book is recognition of the extent to which their world was gendered. The Paget women found their way into libraries, occasionally into lecture halls as lecturers, and up mountains, but vital concepts such as citizenship, access to education, the division of work, and indeed status itself were all profoundly gendered. Beside this overarching fact, the debate as to whether such women led active or passive lives becomes secondary.

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KATHLEEN E. MCCRONE. *Playing the Game: Sport and the Physical Emancipation of English Women, 1870–1914*. Lexington: University of Kentucky Press. 1988. Pp. 310. \$35.00.

Women's physical emancipation through sport was crucial to their social and political emancipation, according to Kathleen E. McCrone. Athletes provided examples of nontraditional female roles that broke down stereotypes of women as weak and uncompetitive and contributed to the development of a new image of feminine perfection in the twentieth century. McCrone describes the growth of individual and team sports in English women's colleges and girls' schools in the late nineteenth century and shows how this growth influenced the women's movement. From the timid beginning of sports at women's colleges in Oxford and Cambridge to the development of national organizations and national awards for intercollegiate competitions, reformers struggled to overcome societal prejudice against women participating in sport. To break free from a conventional dress code, for instance, was difficult because dress was a potent symbol of society's

ideal for women. Reformers challenged the ideal that young women should be frail, compliant, and self-denying by suggesting that they should develop such masculine attributes as strong muscles, physical stamina, and team spirit.

Although it is always difficult for reformers to challenge accepted beliefs, those concerned with women's physical education made their task more difficult by their own actions. To explain this, McCrone extends an argument made by other historians of nineteenth-century women's education to suggest that teachers of women's physical education in England placed themselves in the same double bind as reformers of women's education in general: while they demanded similar education for girls as for boys (and for women as for men), at the same time they insisted that such education not make their students less feminine than they otherwise would have been. As a result, reformers often undermined their own goals by constraining the women on whose behalf they worked. "In sport," writes McCrone, "it was accepted that women should stop at a certain point in order to preserve their feminine image and conform to women's assumed or desired physical limitations" (p. 284).

This book is well-crafted and informative, synthesizing material on the development of sport and physical education and on training for physical education teachers. By choosing not to compare the development of sport for girls and women to its development for boys and men, however, McCrone lost a great opportunity. Had she done so, she could have examined in more detail how far the role of athleticism, which defined masculinity and linked men's performance in war to their performance in sport (as discussed, for instance, in J. A. Mangan's *Athleticism in the Victorian and Edwardian Public School* [1981]), impinged on the struggle to establish sport in girls' schools and women's colleges. She could have investigated the likelihood that some opponents of athleticism for boys found it easier to voice their opposition by writing against athleticism for girls and thus not attack head-on the dominant ideology of male society. Men and women who opposed athleticism seem to have played an important role in influencing women educators to subordinate competitive sport to intellectual development, in contrast to the valorization of athletics rampant in boys' schools at the end of the nineteenth century.

Men as well as women benefited when women's colleges and girls' schools avoided the excesses of athleticism. In the 1880s and 1890s, these institutions developed a new curriculum that combined physical education and sport and a new model for training physical education teachers. Emphasis was placed upon individual well-being as well as team spirit. Not until the twentieth century were similar innovations adopted by the boys' public schools.

Despite McCrone's modesty in interpreting her re-

search, her book provides valuable insights into the social history of the period.

JOAN N. BURSTYN
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DEREK JARRETT. *The Sleep of Reason: Fantasy and Reality from the Victorian Age to the First World War*. New York: Harper and Row. 1989. Pp. 233. \$22.50.

This book is concerned with how English-speaking societies (primarily Great Britain, although nominally North America) regarded fantasy and reality during the eighty years from the accession of Queen Victoria to World War I, a period that Derek Jarrett considers "one of history's great turning points" (p. 3). The fantasies in question are mostly the anthropomorphic frameworks of nineteenth-century Anglican theology and popular preaching, although vampires and Sherlock Holmes also are not forgotten. Methodological problems immediately arise from the question of how a historian studies what a society believes to be real, when, at least from that historian's point of view, it is fantasy. Jarrett rejects the methods of either religious history or the history of ideas and opts for a psychoanalytic approach. Dismissing Freud, he finds Jungian psychoanalysis more promising "with its notion of the collective unconscious and its emphasis on myths and universal archetypes. Jung's impact on literary critics has certainly been greater than that of Freud. And since the psychoanalyst and the literary critic are both in the business of assessing fantasies the historian of fantasy should perhaps look to their disciplines" (pp. 3-4).

As a purported historical study, this is both a confused and a confusing book. It attempts to deal with a society's fantasies over nearly a century without ever articulating basic questions that any historian of such a subject should have put to himself or herself and his or her readers, namely, what part of the society accepted the fantasies in question and what part did not and why and what criteria is the historian using to decide what is fantasy and what is not. Jarrett's stance is almost naively positivistic, presuming that all religious faith is fantasy, that all religious faith is the same, that the anthropomorphisms in which faith is articulated are identical with the objects of that faith, and that there is no line between faith and superstition. Before one attempts to write a history of the expressions of a society's collective unconscious, one has to deal with the issues of how representative of the society as a whole these expressions are and with the historian's own stance vis-à-vis these matters. Jarrett fails this test.

Moreover, his use of data is so highly selective that it requires some justifications. In spite of his claims to depend on Carl Jung, his method is more anecdotal than analytical, and, although many of the anecdotes are fascinating, it is often unclear why these are used while others are omitted or what many of them mean in terms of the book's intent. For instance, Jarrett does

mention the Metaphysical Society, but he ignores both the Synthetic Society (1896–1910) and the London Society for the Study of Religion (1904–), each of which in purpose and composition might have added a significant dimension to his theme. As a work of history, this book is problematic; as a work of one historian's imagination, it is interesting.

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ANNETTE KUHN. *Cinema, Censorship, and Sexuality, 1909–1925*. (Cinema and Society.) New York: Routledge. 1988. Pp. xiv, 160. \$57.50.

Annette Kuhn's book is a pioneering work that forges links between the task of the film critic and that of the film historian. Too often deconstructionist film critics have examined films as isolated texts bereft of their social and historical context. Too often film historians have focused on cinematic development without reference to filmic language. In ably avoiding both perils, Kuhn has produced a slender but richly textured volume that explores the evolution of film censorship in Britain from a multiplicity of perspectives.

Kuhn's book departs from the prevailing "prohibition/institutions" model of censorship, which emphasizes that, for certain subjects, expression in representation was forbidden. The prevailing approach to film censorship as a prohibitive activity emphasizes institutions such as boards of film censors, laws, and legal institutions and treats film censorship as largely a political, rather than a moral, activity. Kuhn recognizes the limitation of the prohibition/institutions model: isolating censorship practices from their social and historical context, ignoring the productive effects of censorship, and limiting one's ability to consider power relations by treating them in a static and overly deterministic manner.

Kuhn has borrowed from Michel Foucault the notion of apparatus, thereby viewing power as a process: "power is exercised rather than possessed" (Foucault, *Discipline and Punish: The Birth of the Prison* [1977], p. 26, quoted in Kuhn, p. 7). Kuhn's emphasis on the fluidity of power is particularly appropriate for examining British film censorship in the period from 1909 to 1925, which was one of great struggle and flux. The British film industry proved curiously ambivalent about censorship: desiring to maximize profitability but simultaneously engaging in a ceaseless quest for respectability that forced a reluctant acquiescence to censorship aimed at improving public perception and at averting more stringent restrictions.

Kuhn's book is enhanced by its use of case studies that reveal how the discourses and practices of film censorship emerged in terms of the social context in which they were generated. For example, the American film *Where Are My Children?* (1916), which under Lois Weber's direction treated the controversial themes of birth control and abortion, was released to favorable

critical and audience reception in the United States but in Britain failed to reach substantial audiences or to achieve commercial success. The film's very construction as a sermon for eugenics appealed to a respectable middle-class American audience and assured its success, whereas in Britain the film's qualities as a "good story" and the prurient interest that its subject might arouse caused its sponsors to fix its meanings for the public, redefining the film as a vehicle of enlightenment and thus denying its use for commercialized leisure and entertainment, that is, as an instrument of pleasure for the masses. Such a propaganda film could not be assimilated into a cinematic discourse that distinguished entertainment from enlightenment; hence, the case study serves to highlight the tension between discourses of public morality and discourses of censorship that accompanied the rise of the propaganda film.

Kuhn's analysis wisely emphasizes the historical fluidity of the censorship process. Her book, however, raises several questions. In the United States, as Linda Gordon has noted in *Woman's Body, Woman's Right* (1976), the birth control movement under Margaret Sanger's auspices sought to achieve respectability by allying itself with eugenicists. Did the British birth control movement follow a distinctive course that would account for the vastly different reception of films such as *Where Are My Children?* that treated eugenic themes? In the United States by the 1920s, the creation of full-length dramatic feature films and the growth of motion picture palaces had attracted a middle-class audience to the cinema. Why in Britain even in the 1920s was the debate over discursiveness still informed by the fear of movie audiences as an ignorant mass? Kuhn's book could also have profited from more detailed analysis of the personnel controlling the various censorship apparatuses in Britain in terms of gender, access to power, and socioeconomic class. But, despite some minor omissions, Kuhn's study weds film criticism and film history in a highly satisfying union.

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MARK SWENARTON. *Artisans and Architects: The Ruskinian Tradition in Architectural Thought*. New York: St. Martin's. 1989. Pp. xix, 239. \$39.95.

In 1852, John Ruskin wrote, "I shall show that the greatest distinctive character of Gothic is in the workman's heart and mind" (p. 22). This idea was the root and branch of Ruskin's defense of Gothic as the only acceptable architecture, as well as an important tradition, reaching into the present century, of moral aesthetics. His beautiful and imaginative prose became an aesthetic language that permeated Victorian society. Nevertheless, Ruskin's influence on contemporary thought is marginal. Historians have often concluded that there were two Ruskins, one a brilliant art critic, the other a crank peddling bits and pieces of backward-looking social theory. A recent Ruskin revival has

resulted in a reevaluation of Ruskin the social thinker, and, hence, the real nature of Ruskin's influence emerges as an unresolved and controversial issue. Mark Swenarton reopens the question of Ruskin's legacy by examining intellectual connections between Ruskin, the British socialist tradition, and five socialist architects: William Morris, the founder of the arts and crafts movement; W. R. Lethaby, writer and Fabian; Philip Webb, architect of the "vernacular" revival; Raymond Unwin, proponent of municipal housing; and A. J. Plenty, founder of guild socialism. Following two recent revisionist studies of Ruskin, Kristine O. Garrigan's *Ruskin on Architecture: His Thought and Influence* (1973) and Peter D. Anthony's *John Ruskin's Labour: A Study of Ruskin's Social Theory* (1983), Swenarton argues that, contrary to the views that Ruskin's intellectual groundings were in Christian, Scottish, or old Tory social thought, Ruskinian thought was rooted in German Romantic-Idealist philosophy. Most important, Ruskin's greatest idea was that architecture is the expression of the character of the worker. In his chapter "The Nature of Gothic" in *Stones of Venice* (1853) and in defense of thirteenth-century Venetian Gothic architecture, Ruskin argued that architecture was beautiful only if the worker was happy and free as both thinker and worker. On the other hand, good architecture in the new industrial age was impossible because the machine and the division of labor had reduced men to mere "crumbs of life" (p. 29).

Swenarton shows how Ruskinian thought became the manifesto of the Working Men's College and the spirit of the "arts and crafts" revolt and gave form to guild socialism and Fabian advances in education. Overall, Ruskin's ideas were the most important non-Marxist influence in the evolution of British socialism. Most successful in bringing Ruskinian thought into daily life were Lethaby and Unwin who, unlike Morris and Webb, abandoned the Ruskinian dictum that good architecture only followed the social revolution. By mixing Ruskin's notions with other intellectual currents, they adopted the opposite view, namely, that architecture could result in good society. Although Swenarton overstates the failure of historians to acknowledge Ruskin as a source of twentieth-century "modernist" thought, he finds a Ruskinian tradition in the work of the German Werkbund, the Bauhaus, Le Corbusier, and Frank Lloyd Wright.

If this brilliant book has a flaw, it is in its weak-hearted and disconcerting conclusion. Unconvincingly, Swenarton dismisses Ruskin's views on work, production, and social regeneration and his spiritual and millenarian concepts as introducing "unnecessary complications into architectural thought" and being both one-sided and irrelevant to the modern world (p. 201). Indeed, is not Ruskin's thesis on the moral superiority of work an attractive alternative to contemporary capitalist and Marxist thought (see Anthony, *John Ruskin's Labour*, chap. 9)? Further, although the author is correct (p. 202) that Ruskin was a poor historian, he was a profound maker of myth, and his great legacy is the

transfer of the myth of the Gothic and the vernacular to the British landscape. All in all, this book makes it difficult to deny Ruskin a major place in British social thought and practice.

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JOHN M. MACKENZIE. *The Empire of Nature: Hunting, Conservation, and British Imperialism*. (Studies in Imperialism.) New York: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. x, 340. \$62.50.

The exotic territories appropriated by British imperialists often came stocked with generous supplies of what John M. MacKenzie refers to as "the animal resource" (p. 299). These vast wild animal populations supported the imperialist enterprise in several ways: they provided ready meat for colonists and adventurers, as well as hides, horns, and trophies for remote markets; they offered a means of (and sometimes a reason for) controlling indigenous human inhabitants; and, on the level of symbol, they celebrated and justified British domination. MacKenzie chronicles the relationship between British imperialists and wild animals in nineteenth- and twentieth-century Africa and Asia. (North America is left out because in Canada imperialists pursued a merely extractive policy; Australia is omitted because MacKenzie mistakenly believes the claims of nineteenth-century imperialists that its native fauna was impoverished.)

This is an important subject and one that has not received its scholarly due, perhaps, as MacKenzie suggests, because the activity of hunting has seemed too frivolous for serious historical examination. It is, however, hard to follow him in blaming P. G. Wodehouse for this neglect, and, in fact, other historians have noticed the significance of the connection between imperialism and wild animals. But no one has given the subject the exhaustive attention distilled in this volume. Based on a wide range of published sources—mostly hunting memoirs, a genre for which the reading public manifested an almost insatiable desire beginning in the late Victorian period, and the official records of Great Britain and its colonies—MacKenzie has reconstructed an authoritative and detailed account of the changing uses to which British imperialists put exotic wild animals. Although strongly subject to variation in specific territories, a general pattern emerges from MacKenzie's analysis: an initial period of unbridled economic exploitation of wildlife was succeeded by a period in which the animals were preserved for more or less ritualized elite hunting and, finally, by a period (not yet ended in most places) of conservation and unarmed tourism.

Superimposed on this pattern is another, which refers primarily to relations between people. MacKenzie persuasively argues that, in colony after colony, official wildlife policy undermined the political and

economic underpinnings of colonized societies. Especially in Africa, increasingly vigorous attempts to manage and protect wild animal populations deprived the indigenous humans of access to what had been a source of food, exchange, and ceremony. Traditional methods of hunting were outlawed; tribal hunters were disproportionately blamed for violations of game laws; ultimately, humans and animals were segregated into separate territories. Wildlife policy could also reflect conflicting interests among the imperialists. One of the most interesting sections of this study concerns the tsetse fly controversy of the early twentieth century, in which settlers wished to sacrifice wild animals to their cattle and elite hunters wished to sacrifice domestic livestock to game.

MacKenzie effectively presents wild animals as pawns in the imperial game. He focuses on the relationship between human groups, colonizers and colonized, often stressing the fact that imperialists tended to protect wild animals at the expense of indigenous people. Although this focus is understandable in the context of his argument, it does lead him to a significant omission. He ignores the possibility that the animals might have some independent interest in the developments that he discusses.

HARRIET RITVO
Massachusetts Institute of Technology

P. D. COATES. *The China Consuls: British Consular Officers, 1843–1943*. New York: Oxford University Press. 1988. Pp. x, 619. \$39.95.

The British Foreign Service in the nineteenth century was consciously and deliberately separated into particular entities. The Foreign Office was distinct from the Diplomatic Service, and both were separated by a wide social gulf from the Consular Service. Within the Consular Service, two special services combining political and economic functions were created, one for the Levant to handle the Eastern Question and one for the Far East that originated in China after the First Opium War. P. D. Coates's book is the first general history of the Chinese Consular Service to be undertaken and has been painstakingly gathered together from over four thousand volumes of consular records retained in the Public Record Office. The only other modern study of the China consuls is a short chapter in D. C. M. Platt's *Cinderella Service: British Consuls since 1825* (1971). Platt's work rests on various parliamentary enquiries undertaken into the Consular Service, and his book reflects their Gladstonian candle-end interest in cutting back a service that was seen to be overpaid and overstaffed. Coates, who is a former consular official with experience in China, will have none of this and in great detail sets out the trials and tribulations of nearly all of the three hundred consuls who served in China. They lived extraordinary lives, and Coates paints a full picture, warts and all, of what it actually meant to be a working official in China. Some prospered, many found-

dered, but very few escaped permanent damage to their health. Coates has succeeded in doing for the Chinese consuls vis-à-vis Platt what Rupert Allason (alias Nigel West) has done vis-à-vis F. H. Hinsley for the British secret service. When Hinsley's official history appeared to be no more than a history of the committees of the service, its members encouraged Allason to write a history of MI5 that revealed in revealing names.

Within its self-imposed limits, this is a good book, useful for reference and full of good anecdotes. But a work of history has to be more than a recital of facts.

The book is divided into three chronological periods dealing with the Treaty Port era from 1843 to 1860, the expanding network in the heyday of imperialism, and the fall of the Manchus and the twentieth-century service up to the end of extraterritoriality in 1943. An appendix provides biographical details of all 309 members of the service appointed between 1843 and 1934. The *raison d'être* of the service was the extraterritorial regime imposed on China to facilitate British trade. The service lasted as a separate entity only as long as extraterritoriality did. The last appointments to the separate service were made in 1934.

RAY JONES
Carleton University

AARON L. FRIEDBERG. *The Weary Titan: Britain and the Experience of Relative Decline, 1895–1905*. Princeton: Princeton University Press. 1988. Pp. xvii, 329. \$29.95.

Aaron L. Friedberg asks how political leaders become aware of shifts in relative power and how they try to respond to them. He discusses two models of assessment and adaptation, namely, the calculative and the perceptual. The calculative relies on collection of statistics relating, for example, to trade performance, government revenue, and the size of a country's armed forces. The perceptual is based on the images possessed by a nation's rulers of both the power of their own state and that of its potential rivals. Friedberg takes Britain's adjustment to relative decline in the period from 1895 to 1905 as a case study with the object of testing the adequacy of the two models.

The historical discussion analyzes how Britain's rulers perceived and responded to the erosion of their country's economic, financial, naval, and military power. Friedberg finds the process of adaptation uneven and not necessarily adequate. Thus, the Conservative governments that were continuously in power during the period failed to provide even the rudiments of an industrial policy that might have helped make British products more competitive in markets where increasingly they were losing out to German and American competition. And, rather than expand defense spending at a time when the international environment was characterized by the rise of predatory states looking for their own place in the sun, Britain followed a policy of retrenchment. Strategy was dictated by the

politico-economic imperatives of low taxation and the night-watchman state. In consequence, Britain was woefully unprepared for the type of war into which it was plunged in 1914.

Using an approach that combined elements of the calculative and the perceptual models, Britain's rulers discussed issues with statistics that were misleading (the trade figures) and refused publicly to acknowledge the passing of maritime supremacy. Given that debates were often conducted with inadequate data or on the basis of unrealistic assumptions, the conclusions reached tended to be inappropriate. Friedberg's finding is that Britain's experience shows how slow political leaders may be to understand the nature of shifts in the distribution of power within the international system.

This is a very rich book, extremely well researched and written throughout with great clarity. It is full of thought-provoking ideas and is a welcome addition to the lively debate about Britain's decline and the rise and fall of great powers. There is, however, one minor and one more significant flaw. First, Friedberg understates the influence of the National Efficiency movement. Second, and more seriously, he underestimates the extent to which a fully appropriate response to decline, which he justifiably argues would have implied a more interventionist state, would have flown in the face of the liberal economic priorities of the ruling elite. Those priorities reflected the interests of an establishment whose wealth and power were products of Britain's longstanding international financial role as the world's banker. But a commitment to free trade might have undermined manufacturing industry even as it allowed the City of London to flourish. This possibly was spotted by Joseph Chamberlain, who understood that a strong industrial base was a prerequisite of strategic power, and it is arguable that he was defeated not because he lost an argument but because the adoption of his tariff reform program (which did not, by 1903, aim at an imperial *zollverein*, as Friedberg suggests) would ultimately have involved the substitution of the current ruling class by one rooted in manufacturing industry. Friedberg's rather idealist approach, however, means that he dodges the question of how far the response of Britain, or indeed of any major power, to relative decline requires the resignation or replacement of its governing elite.

SCOTT NEWTON
University of Wales College,
Cardiff

JON TETSURO SUMIDA. *In Defense of Naval Supremacy: Finance, Technology, and British Naval Policy, 1889–1914*. Boston: Unwin Hyman. 1989. Pp. xix, 377. \$60.00.

It was one thing for the Royal Navy in the first decade of the twentieth century to develop the all-big-gun warship but quite another to see to it that the guns shot straight at long range. Right to the end of World War I,

the Royal Navy's fire control was markedly inferior to that of the German High Seas Fleet.

Yet there was a fire control system far superior to the Admiralty's contemporary lash-up. This book tells an interminable, complicated, depressing story, in some respects a classic tale, of a brilliant, bohemian inventor, Arthur Pollen, up against service officers and a bureaucracy unequipped through lack of training, ignorance, and lack of adequate staff to deal with innovation. Admiral Sir John Fisher and Frederick Dreyer, those putative technological progressives, also proved stubborn obstructionists. And here Jon Tetsuro Sumida makes an impressive contribution to an understanding of the naval policy and technology of the time, free of the Fisher hagiography so evident in previous works in the field.

The Royal Navy paid a heavy price at Jutland for its gunnery failings, as well as for its inadequately protected battle cruisers and its defective ammunition: three British battle cruisers blown to pieces and not one German modern capital ship destroyed. With the breadth and depth of opposition, it is not surprising that only in the mid-1920s did the navy, prodded by Admiral Lord Beatty, finally adopt the Pollen fire control system.

The Pollen system was anything but a piece of technology limited to the era of the now nearly vanished big-gun armored warship. Pollen's system used differential analyzers, thus pioneering not only the computer but also (for better or worse) systems analysis. Sumida also outlines John Fisher's well-known obsession with the battle cruiser, which he believed would emerge as the capital ship of the future, not his own HMS *Dreadnought*. Fisher's final legacy to the Royal Navy was the misbegotten "large-light cruisers" *Furious*, *Courageous*, and *Glorious* (known even at the time as the "Curious," "Outrageous," and "Uproarious"), fortunately soon converted to aircraft carriers. (And, as late as 1941, the Royal Navy's latest battle cruiser, the giant *Hood*, blew up while in combat with the *Bismarck* and the *Prinz Eugen*.)

Sumida also details the financing of Royal Navy expansion and describes some of the parliamentary maneuverings over naval matters of the time. But the heart of the volume is the labyrinthine struggle of Pollen and his supporters inside the Royal Navy to adopt a far superior technology. Perhaps Sumida tells us more about the struggle than we really need to know. The characters, including Pollen, are shadowy, and the reader would have profited from more analysis of personalities. Sumida only touches on the motivation of Pollen's adversaries, who, after all, had recently participated in one of the greatest technological revolutions in modern naval history. Perhaps Pollen himself should have the last word on them: "there is no conservatism like that of the progressive. It was not the duffers, but the greatest astronomers in the world, who found Galileo's discovery hard to swallow" (p. 124).

This is a volume for students of modern naval history and of twentieth-century technology, and, al-

though they will have to work at it, they will know that they have read the definitive work on the subject.

STANLEY SANDLER

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STEPHEN G. JONES. *Sport, Politics, and the Working Class: Organized Labour and Sport in Inter-War Britain*. (International Studies in the History of Sport.) New York: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. xi, 228. \$45.00.

This book is an exemplary study of the relationship between organized labor and sport in Britain during the interwar period. In conjunction with massive research in the records and reports of labor and socialist organizations, government departments and agencies, and official inquiries, as well as contemporary newspapers and periodicals and extensive secondary works, Stephen G. Jones employs Marxist social theory in a flexible, nondoctrinaire manner to illuminate sport as an aspect of capitalist and class domination in state and society and as one of the many sites of struggle involving socialist intervention.

The book has eight chapters. The first considers the meanings of sport in society and outlines several theoretical approaches to sport to enable readers to situate the historical discussion that follows in a theoretical framework. Chapters 2 through 5 explore the role of labor groups in the evolution of modern sport in the late nineteenth century, the main trends in the organization and provision of working-class sport between the wars and the effects on them of commercialization and commodity production, and the Marxian and social democratic initiatives in sport with reference to aims, institutions, policy, antagonisms, and achievements. Concluding the work are examinations of the national and international aspects of socialist involvement in sport, specifically the relationship among working-class politics, the state, and sporting reforms and British participation in European workers' sport—the only subject whose significance the author exaggerates.

Jones sees sport as an essentially middle-class, male preserve related to other forms of bourgeois and patriarchal control. Normally a conservative institution embodying and re-creating the principles and practices of class and gender inequality dominant elsewhere, sport can thus act as the ultimate idiom of conformity. At the same time, however, it can be an important deviant activity with considerable potential for social disruption when used by subordinate groups as a means of challenging existing social arrangements. This is the book's main theme.

Throughout the work, the author treats sport as a complex and ambiguous cultural phenomenon operating within Britain's overall socioeconomic formation of work, politics, and community. He carefully avoids exaggerating sport's importance to the working class (many of whom were unaware of the workers' sport

movement) and to organized labor—to whose larger purposes it was incidental. At the same time, however, he presents working-class sportsmen as dynamic actors in a larger social drama. As producers and consumers, he argues, they used various forms of collective self-determination to imprint sport with their own distinctive meaning and traditions. Although members of the working class had little control over the business and administrative sides of a sporting world that was largely developing within the context of capitalism and class inequality, they refused to accept a middle-class ideology of sport and, moreover, criticized a social system that denied them adequate sports facilities, playing grounds, and open spaces. As a result, Jones concludes convincingly, socialist bodies were able to use sport as an effective instrument for resisting the ruling interests' domination and for articulating and achieving their members' own needs, interests, and aspirations.

The author himself recognizes that, as a pioneering work on a hitherto unexplored subject, his study raises a number of issues needing further theoretical and empirical investigation, not the least of which is the female dimension that he acknowledges as important but chooses not to develop. Jones, alas, cannot do the investigating himself, having been tragically killed in a road accident, at the age of 30, shortly after completing the manuscript. It is to be hoped that his family and professional admirers will derive at least some comfort from the knowledge that, by this and his other fine works, Stephen Jones achieved a degree of immortality of which many, more senior scholars would be proud.

KATHLEEN E. MCCRONE
University of Windsor

RICHARD COCKETT. *Twilight of Truth: Chamberlain, Appeasement, and the Manipulation of the Press*. New York: St. Martin's. 1989. Pp. x, 229. \$35.00.

Those who may wish for a rehabilitation of Neville Chamberlain and British foreign policy from 1937 to 1940 will derive no comfort from this book. Its central themes, consistently elaborated throughout, are that the Chamberlain government shamefully—and sometimes treacherously—manipulated the British press in order to silence criticism of Adolf Hitler and Benito Mussolini (a seeming prerequisite of any settlement with them) and put the best possible face on its dealings with the dictators and, further, that many proprietors and editors, equally shamefully and treacherously, succumbed to manipulation (for various reasons) to the point of losing touch entirely with public opinion and becoming “not so much the watchdogs of democracy as the harlots of democracy” (p. 187). It is impossible to read this book without feeling a sense of outrage at the chicanery exhibited by both government and press. Every chapter effectively reinforces a viewpoint that I have long embraced, namely, that any evaluation of Chamberlain and appeasement must rest not simply on the nature of the policy alone but also on the manner of

its execution and the behavior of its proponents in the pursuit of their purposes. In these regards the appeasers do not fare well.

Chamberlain's studied manipulation of the press began with the visit of Lord Halifax to Germany in late 1937, expanded with Anthony Eden's resignation from the Foreign Office and the Czechoslovak crisis, and reached its apex in a vast exaggeration of the positive aspects of Munich and the absurdly inflated optimism of early March 1939 about the prospects of peace. The invasion of Prague marked the onset of disillusionment in some newspapers, but it was not until the outbreak of war that many editors and proprietors, having been made to look foolish on previous occasions, began to react against their former pusillanimity with suspicion and resentment toward the government and to express mounting dissatisfaction with Chamberlain's conduct of office. Meantime, the prime minister, believing that by controlling the press he was capturing public opinion, became trapped in a twilight world of self-delusion that rendered him incapable of accepting criticism or of understanding that there might be opposition to his plans that sprang from grounds more substantial than party malice or mental illness.

Gagging the Foreign Office News Department and operating his own press office (and press lobby) at Downing Street, Chamberlain also established extensive private and unofficial contacts with proprietors and editors, who received frequent appeals (sometimes with overtones of intimidation) for greater self-censorship. Even the Labour *Daily Herald* fell into line, its owner deeply concerned about the potential loss of advertising that might accompany suggestions that anything other than peace and security were in prospect. Sometimes conflict arose between editors and proprietors, but proprietors usually prevailed. The stubborn disregard of editors for the reports of their own correspondents in the field, particularly evident during the Czech crisis, is one of the more despicable aspects of the story. (One exception was Arthur Mann of the *Yorkshire Post*, perhaps the most consistent critic of appeasement.) The result was a conspiracy of silence that obscured deep divisions over Chamberlain's policy in Whitehall, Westminster, and the country at large—a silence that effectively precluded both the articulation of any alternative policy and the presentation of facts and figures to the British public that might have prompted consideration of alternative policies. Thus, the later defense of appeasement as the only policy possible because the country was not sufficiently educated to support a stand against the dictators is specious in the extreme, considering that Chamberlain did his utmost to ensure that there was no "education" of the country.

This well-written volume is solidly researched and documented, using official government records, revealing diaries, and the manuscript files of leading government officials and newspaper people. Its implications reach far beyond Chamberlain and appeasement, demonstrating that control of the press not only subverts

democracy but eventually corrodes the minds of those who exercise it.

WILLIAM R. ROCK
Bowling Green State University

BRIAN JENKINS. *Era of Emancipation: British Government of Ireland, 1812–1830*. Kingston, Ontario: McGill-Queen's University Press. 1988. Pp. 383. \$37.95.

The greatest difficulty I had with this book was putting it down. Brian Jenkins is a skillful storyteller with a lucid and graceful style. He is working with a rich documentary base, which he puts to full advantage. Although plagued with more than the usual number of typographical errors, the volume is handsomely printed and bound—a credit to both author and publisher.

The purpose of the book is to analyze the politics of the British government of Ireland from 1812 to 1830. More specifically, Jenkins investigates the politics and circumstances leading up to the Emancipation of Catholics. More generally, he helps answer the larger question of why the union of Ireland and England failed.

The contrast with Wales and Scotland is striking. Over the course of the seventeenth and eighteenth centuries, the integration of Wales and Scotland with England was completed. The final stages consisted of the union of the Scottish and English parliaments in 1707 and the defeat of the Rebellion of 1745 with the confiscations, executions, banning of the kilt, and abolition of heritable jurisdictions that followed. By the late eighteenth century, Scotland was perhaps more effectively governed from London than parts of England itself.

Meanwhile, Ireland had a separate administration with a separate parliament. When the Act of Union was passed in 1800, it was hoped that Ireland would follow Wales and Scotland and become an integral part of the United Kingdom. Why it did not is a long story to which Jenkins makes a significant contribution by identifying the sources of Irish "exceptionalism" in the period that he is studying. Four factors stand out in his analysis. First, the decision to maintain an Irish executive in Dublin with a viceroy made the governance of Ireland distinctly different from any other part of the United Kingdom. Second, Irish poverty, which in its scale surpassed even that of the Scottish Highlands, simply could not be mastered by any Irish administration, whatever its political composition. Third, the magnitude of popular unrest and the routine violence of Irish society presented problems that the popular disturbances in England in this period could not match. And, finally, the civil disabilities imposed on Catholics effectively excluded them from participating in the United Kingdom on an equal basis, and the bitterness that these disabilities created made their integration impossible even as civil equality was achieved. Emancipation was the first step in the separation of Ireland

from the United Kingdom rather than a step toward full integration.

The book has its weaknesses and blemishes. In places the author digresses; for example, he provides us with several pages on economic and social conditions in England. He adopts the most confusing convention for acknowledging sources: all we are given are footnotes at the end of each paragraph, and we have to guess at the sources for most quotations, indeed even the authors of some quotations. More seriously, his research is based largely on the diaries and correspondence of the lord lieutenant, the home secretary, the chief secretary, and his undersecretary. This not only leaves out many sources; it also means that we are getting a biased perspective on what happened. Nonetheless, this study is one of the best books on Ireland to appear in this decade and essential reading for anyone interested in nineteenth-century Ireland.

SAMUEL CLARK
University of Western Ontario

ALVIN JACKSON. *The Ulster Party: Irish Unionists in the House of Commons, 1884–1911*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1989. Pp. x, 359. \$69.00.

The Ulster party, led by Colonel Saunderson and Lord Arthur Hill, was born in 1884 when Irish loyalist representatives feared the Tories had sold them out in negotiations over franchise reform and distribution. Although it never had a comprehensive political program, the Ulster party continued largely as a parliamentary movement with Irish objectives until the early twentieth century, by which time it had been transformed into a constituency movement dominated by Ulster localism. Alvin Jackson's stimulating and detailed study of this parliamentary faction describes the origins of the Ulster party and examines its personnel, its contributions to debate, and its organization and discipline. He treats its policies and the relation of those policies—Home Rule, land, local government, and Roman Catholic higher education—to the party positions of the regnant Conservatives. Turning then to the constituencies, Jackson examines questions of funding, the selection of candidates, internal debates, and local politics. Finally, he discusses the transformation of the party from one based in Westminster to one based in Ulster by describing the party's conflicts with constructive Toryism, George Wyndham, and the emergence of Ulster Unionism militancy.

A small group numbering never more than twenty-three MPs, the Ulster party was joined by a few Irish members sitting for English Tory constituencies. In the beginning the Ulster party consisted largely of landed gentlemen such as Edward Saunderson and Arthur Hill, but, with the general decline of landlordism in Parliament during the 1890s, the proportions of lawyers and other professionals as well as merchants and businessmen grew. In this the party was quite unlike

the Irish Nationalist party or even the Tories, as a matter of fact, and came to resemble the social structure of the British Liberal party. Although the Ulster party did not wholly agree on policy and was not dominated by marked discipline, the stable political conditions from 1885 to 1918 and relatively safe seats in Ulster allowed it to return members who were more mature in years and in parliamentary experience. In terms of their social background, educations, memberships in London's clubs, and places in Tory governments, members of the Ulster party found it possible in the 1880s and 1890s to assimilate themselves into the workings of British parliamentary politics in alliance with Salisbury's Tory party.

Naturally the Home Rule years, 1886 and 1893, drew Irish loyalists closest to their Tory allies. Even on this issue, however, they were not absolutely at one. Unlike the Tories who regarded Home Rule as a threat to imperial unity, the Ulster party conceived of Home Rule as an attack on the Act of Union. Irish loyalism was a species of parochial patriotism, not a brand of imperialism. Land questions during periods of Conservative government and constructive unionism forced wedges between the Tories and their Ulster allies. Irish loyalism was itself unstable, however, and changes in the composition of the parliamentary party as well as the land issue forced divisions even within the Ulster party.

Despite disagreements, an alliance between the Ulster party and Salisbury's Conservative party endured during the Home Rule years from 1886 to 1893. The new men from Ulster at Westminster, especially businessmen with weaker connections to British society and politics, the development of more aggressive constituency politics, and the crisis over Wyndham's chief secretaryship led to growing ruptures with Arthur Balfour's Conservative party. The Ulster party was strongest and most influential in British politics when it could claim to stand for more than Ulster alone. As the Irish loyalist movement became socially narrower and more militant, it moved to the periphery of politics, where it now stands. This book, although it is about a small parliamentary party, makes an important contribution by tracing the relationship between political recruitment and the development of policy in Irish politics between 1884 and 1911.

W. C. LUBENOW
Stockton State College

DIANA M. HENDERSON. *Highland Soldier: A Social Study of the Highland Regiments, 1820–1920*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1989. Pp. viii, 336. £22.00.

Dubbed by Diana M. Henderson as "a unique social history," this highly specialized, detailed monograph with haphazard coverage of some social themes is based on an Edinburgh dissertation of 1986. Recall that in the early modern period Scots had served in Continental

armies such as the Dutch, French, and Swedish and that Scotland was geographically and politically divided for ethnic and Jacobite reasons into Highland and Lowland regions. Scottish Highland regiments such as the "43rd" or the "Black Watch" (which exists today) were formed early in the eighteenth century. A martial spirit and aura of heroism revolved around Highland soldiers, Henderson contends, and, in the century before 1820, "the repressive attitude towards the 'rebellious' and 'uncivilised' North changed to at least one of romantic curiosity frequently tinged with a jealous respect" (p. 9). Nevertheless, we are uninformed about revolts and mutinies involving Highland regiments in the late eighteenth and mid-twentieth centuries. Hew Strachan calls the Highlanders of the eighteenth century "pillagers," comparing them to Cossacks and Croats.

In 1820 there were ten Highland regiments. Demobilization was the fate of the victorious British army after the Battle of Waterloo, but large numbers of troops remained to garrison the empire. The army was subject to a series of reforms, for example, those associated with Edward Cardwell, R. B. Haldane, and amalgamation during this period. These Scottish regiments often served abroad. They also were used to quell disturbances in Ireland and England but not in Scotland.

For unconvincing reasons Henderson focuses on the century from 1820 to 1920. She is more persuasive when she insists that Highland regiments were unique and distinctive in recruiting methods, in composition, in dress particularly because of the kilt, and in the use of music as an integral part of regimental routine in peace and war. Henderson is enthusiastic and most effective in her description of regimental music. Use of the bagpipe, bugle, fife, and drums was the center toward which regimental routine, structure, and life was oriented. Musician-soldiers enjoyed special status, and, on occasion, German civilian band masters were hired to improve quality. The repertoire was expansive, for example, "Auld Lang Syne," "The Girl I Left Behind Me," and "My Native Highland Home."

She concludes with a catch-all chapter that incorporates such diverse factors as cleanliness, education, discipline, punishment, drunkenness, religion, sexual relations, and the role of wives and children in regimental life. None are treated adequately or satisfactorily. A clear conception of the Highland soldier is difficult to envision.

It is dull reading. Trivia is substituted for sociological analysis, and the coverage is irregular. There is a chapter recounting excruciating details of finances and expense accounts of individual officers. Indeed, one-fourth of the book is devoted to "the character and nature" of Highland officers. There is comparatively less on the rank and file.

The research is impressive. There are many tables, charts, and maps showing locations of barracks and stations in Scotland, Ireland, Canada, Gibraltar, and India. Extensive use is made of diaries, letters, regi-

mental records, War Office documents, parliamentary papers, and oral history archives. She cites most of the best writers—Hew Strachan, Edward Spiers, Gwyn Harries-Jenkins, Myna Trustram, Alan Skelley, and Brian Bond—but misspells two of the names, and she relies most appropriately on the *Journal of the Society for Army Historical Research*. This book is recommended for those interested in detail of certain aspects and limited time periods of the British army.

EUGENE L. RASOR

Emory and Henry College

KRISTEN B. NEUSCHEL. *Word of Honor: Interpreting Noble Culture in Sixteenth-Century France*. Ithaca: Cornell University Press. 1989. Pp. xiv, 223. \$27.95.

The sharing of the same bed by the prince de Condé and his captor, François duc de Guise, in 1562—what Kristen B. Neuschel terms a "bizarre ritual" in her preface—is rendered comprehensible by the end of Neuschel's penultimate chapter. To accomplish this, she studies the relationships between Louis de Bourbon, prince de Condé, and the nobles in his governorship of Picardy; she then expands the insight thus gained to all sixteenth-century French nobles.

Beginning with a lengthy bibliographical essay in which she locates herself as a not uncritical user of the clientage model, Neuschel examines the relationship between Condé and his clients during the early Wars of Religion: one-third of Condé's initial army came from Picardy and neighboring territories; the remaining two-thirds came mostly from the Bourbon lands to the southwest and from the Châtillon lands around Paris. But this apparent agreement with the clientage model dissolves when acts of specific nobles are studied: Picard nobles joined the rebellion and left it as their own circumstances, not Condé's, required. In peacetime, similar incongruities are revealed in the exchange of honor and favor: although this exchange frequently occurred between patron and client, it was not restricted to them but permeated noble society in its entirety.

Neuschel argues that nobles' awareness of their public roles, responsibilities, and authority transcended their private relationships. Honor was both the proof of their status and the source of their political legitimacy, and the preservation and increase of this honor was the intent of both acts and words. She describes noble culture as an essentially oral one, lacking the intricacies of structured thought; repeated courtesies recognizing the status of the actor and the recipient played a strategic role in establishing relationships among nobles, as well as between all nobles and the state. Thus, Guise and Condé both had reason to share the best bed in the castle despite their enmity: to emphasize the captive's honor added to the honor of the captor, and, to the captive, honor ensured political and personal consideration; honoring the captor estab-

lished the right of the captive to render such honor, stating a claim to continued political participation.

Applying this insight to clientage and social structure, Neuschel discovers that propinquity and participation in a noble's household more often led to clientage than did vassalage: clientage grew out of the honor that was regularly and repeatedly given and received between neighbors and between members of a household. Structured feudal contracts rarely led to clientage, although clientage was sometimes rewarded with a fief. Neuschel has thus modified the clientage model by studying the *mentalité* of nobles; she argues that nobles' perceptions put restraints on the meaning of the patron-client tie. Generally, noble clients were not creatures completely dependent on the patron.

Neuschel has studied the imperfections of the clientage model to learn what caused them, and she has thereby constructed a rudimentary definition of nobles' *mentalité*. Despite the uneven preservation of primary historical sources discussed in the bibliography, she has written an intelligent critical essay; however, the argument does not fully persuade. One hopes Neuschel will extend her insight to a wider territory and a more complete range of evidence. It might also prove beneficial for her to check contemporary legal and social theorists, not for direct evidence concerning nobles but for an assessment of how the non-oral culture interpreted the oral.

GERARD F. DENAULT
Cambridge, Massachusetts

A. LLOYD MOOTE. *Louis XIII, the Just*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xiv, 401. \$29.95.

The fourteen sections of this biography follow Louis XIII's life with a roughly equal emphasis on each part of it. His childhood was recorded in extraordinary detail by his physician, Jean Héroard, and, in interpreting this material, A. Lloyd Moote steers a judicious course between the ideas of Madeleine Foisil, who saw Louis's early childhood as "a happy experience in a supportive environment," and Elizabeth Marvick, who viewed Louis's early years as the disastrous preparation for his later misfortunes. Moote concludes that "Louis was a difficult child, and his well-intentioned entourage employed remedies that . . . stunted him physically and emotionally" (p. 38).

The difficult child became a tormented young man, beset by physical and emotional problems that, because of his exalted position, he could share with few people. Moote gives a lucid and terrifying account of Louis's marital problems, noting that Richelieu was among the few contemporaries who fully understood his master's travails. In the midst of all of these personal difficulties, though, Louis valiantly learned and lived out the *métier du roi*, particularly in his remarkable range of travels, set out here in two effective maps. Moote is much concerned to elucidate the relationship between Louis

and Richelieu, concluding that Richelieu in fact obeyed the king's will by advancing proposals and implementing policies that he knew to be in accordance with Louis's (very strongly held) principles.

There is a sympathetic and interesting analysis of Louis's relationship to the Catholic Reformation and an attempt to show that Louis's generally understated style was reflected in the period's art. The section on the coming of the "warfare state" after 1635 is interesting but could be much fuller; these were developments, after all, that would centrally affect French history down to the end of the *ancien régime*.

Moote has nothing to say about French ventures overseas and, as is his right, chooses not to dwell on any aspects of the material culture of the reign; we learn nothing, for instance, about the cabinetmakers or gunsmiths who so flourished under the patronage of Louis and his friends and who fashioned artifacts that still astonish us by their elegance and craftsmanship. The book is in general well produced, although the italicization of French words is extremely erratic, that is, generally lacking but occasionally excessive. There are very few misprints, and, if I note two of these (p. 126, "Bergérac," p. 204, "apsects"), it is chiefly in the hope that a paperback version of the work may soon emerge in which they may be corrected. For this is a book that, with its elegance and balance, deserves a wide readership, particularly among students who presently lack any adequate biography of this important king.

DAVID BUISSET
Newberry Library

DAVID G. TROYANSKY. *Old Age in the Old Regime: Image and Experience in Eighteenth-Century France*. Ithaca: Cornell University Press. 1989. Pp. xiv, 260. \$31.50.

This book comprises a set of loosely related chapters that David G. Troyansky has designed to give different "perspectives" on both the representation and the experience of old age in France in the period from the seventeenth to the early nineteenth centuries. The book spills over the chronological limits announced in the subtitle, especially in chapter 8, where the author devotes over half of the text to old-age policies of the French revolutionaries.

The first part of the book focuses on an exegesis of representations of the old in seventeenth- and eighteenth-century painting, literature, and theater. In this section of "cultural history," Troyansky treats his sources as revelatory of attitudes of France's social elite. The title of chapter 3, "From Ridicule to Respect," indicates the author's belief in a sea change in attitudes toward the old that occurred around the middle of the eighteenth century. In chapter 4, "From Augustinian Retreat to Ciceronian Retirement," Troyansky uses two interesting models or ideals of old age to broaden the contrast between seventeenth- and later eighteenth-century attitudes. Whereas in the seventeenth century, old age was viewed essentially as a preparation for an

edifying death, Enlightenment writers, with their secular values, approached old age as a life-course stage of substantive interest and possible joys—consisting, like most other stages, in being surrounded by family and friends.

In other parts of the book, Troyansky turns to sources and methods of social history. Chapter 1 outlines basic statistical data on French mortality and age composition. Chapter 6 contains an examination of the condition of the aged in two villages in the second half of the eighteenth century (one in Provence, the other in the North). Chapter 7 investigates the fate of the aged in France's large cities, particularly their relations with hospices and hospitals.

Troyansky is at his best in the "social history" chapters. His use of notarial records for fleshing out the relations of aged and young in Provence is particularly interesting, although the author fails to tell his readers how and why he chose the villages or the years of study that he did. His analysis of the aged's relationship with French medical institutions reveals once again the fragility of the urban family. There is much to admire in the range of sources that the author has explored.

Yet the reader is left bewildered by the lack of integration of the different chapters and puzzled by the organization of the text. The kinds of attitudinal changes that Troyansky has identified in elite culture in the second half of the century are simply dropped from consideration when it comes to understanding the lives of the lower classes, both rural and urban. The author has concentrated on attitudinal or "mentalities" material relating to the upper classes, while using "social historical" material on the daily lives and experiences of the poor. In chapter 8, Troyansky attempts to bring attitudes toward the aged together with sources on actual living conditions by studying revolutionary policy, but this effort at integration comes rather too late. Thus, the book is best considered as a set of case studies on selected topics in the history of the aged, with those on the rural and urban poor of most interest.

KATHERINE A. LYNCH
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JEAN-JACQUES CLÈRE. *Les Paysans de la Haute-Marne et la Révolution française: Recherches sur les structures foncières de la communauté villageoise (1780–1825)*. Foreword by MICHEL VOVELLE. (Ministère de l'éducation nationale; Commission d'histoire de la Révolution française; Mémoires et documents, number 44.) Paris: Comité des Travaux Historiques et Scientifiques. 1988. Pp. 397. 300 fr.

In this solid study of the revolutionary transformation of property rights and the subsequent changes in the distribution of land ownership, Jean-Jacques Clère makes a fundamental contribution. For Clère the main issue is the connection between the revolution and the transition from a feudal to a capitalist order.

Clère analyzes several sources of peasant frustration and indignation: the clearing of uncultivated land (most of which was common land); attempts to eliminate late summer and autumn grazing rights on privately held grasslands; appropriations and usurpations by seigneurs of communally owned woods; and the payments due to the seigneurs. The seigneurs and their agents, he writes, had adopted the ideas of the physiocrats favoring individual ownership and profit-motivated exploitation. Well-to-do peasants could benefit from freedom to manage their land and from the upward trend of prices, but most peasants were profoundly alienated.

In July 1789, revolutionary peasant actions against feudal rights occurred in two areas: east of Langres at the border of Franche-Comté and around Chaumont in the valleys of the Marne and the Rognon. Thereafter peasants generally refused to pay dues to seigneurs. In 1792 and 1793, villages began trying to recover the commons that had been appropriated by seigneurs years before. The question of whether to divide up the commons often split the villages on economic lines. Most villages in the Haute-Marne did not divide common land.

Between 1790 and the end of 1799, the administrations in the Haute-Marne sold 60,675 hectares of nationalized arable and pasture that had formerly been the property of ecclesiastical institutions, the royal domain, or émigrés. Of that land, 12,050 hectares went directly to peasants (not counting the *fermiers* or *laboureurs*, who did little or no manual labor). Much of the rest, bought at first by members of the bourgeoisie, was subsequently divided into small lots and resold to peasants. That practice was not limited to the original buyers of nationalized land. It continued for decades. Peasant proprietors became more and more numerous, and their holdings became smaller and smaller. By the time of the first cadastre (1836, on the average), peasants owned 43 percent of the land instead of the 17 percent that they had owned fifty years earlier.

Clère introduces some interesting source material not often used by other historians (*monitoires*, records of litigation and arbitration, and mortgage loan registers, for example). In that way, as in others, his book is a model.

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PHILIP MANSEL. *The Court of France, 1789–1830*. New York: Cambridge University Press. 1988. Pp. xi, 224. \$44.50.

This book by Philip Mansel complements his study of the court of Napoleon I, *The Eagle in Splendour* (1988). His new work concentrates more on political and administrative aspects without excluding social and cultural affairs, for Mansel defines the court as "the

personal households of the monarch and his family, and the palaces and what happened in them" (p. 2).

Mansel describes Louis XVI's court as simple but formal. It was compatible with the early revolution and was so much a center of the life of France that there was no effort to destroy it while the monarchy lasted. Thus, the court itself was not a significant factor in the fall of the monarchy.

Napoleon I's court differed considerably from that of the Bourbons. Admission was based essentially on official position rather than on birth or royal favor. And Napoleon's court focused on the function of the monarch rather than on his person, an attitude Mansel describes as "ultra-monarchical" (p. 70). Under Napoleon the court gained political importance and influence over the army. The danger inherent in linking power to the court rather than to independent institutions was realized, however, as Napoleon's officials deserted him in the spring of 1814, a consequence that I feel Mansel has not adequately stressed.

After the revolution the court emerged stronger than before. Louis XVIII's court again separated the court and officialdom and combined elements of the Old Regime, the Second Empire, and the émigrés. But many courtiers opposed conciliation, causing hatred of the court, especially by voting against government measures in the Chamber of Peers. In this context Mansel unfortunately minimizes the relationship between the ultra-royalists in the Chamber and the comte d'Artois and his followers during the political crises of 1820–21.

In November 1820 the court experienced the only complete reform in its history, beginning the decade Mansel considers the "golden age" (p. 129) of the French court. The court hierarchy was regularized. Official position, as well as political, social, and financial status, became the basic means of access to the court. One goal was to gain support of the middle class and of the Napoleonic nobility, and, according to Mansel, Napoleonic influence permeated the court, not waiting until 1830 to reappear. Contrary to the usual view that the court now caused much dissatisfaction, Mansel insists it had only minimal influence on Louis XVIII and Charles X.

Louis Philippe abandoned the popular form of the court of Napoleon and the Bourbons, thus losing public support. This weakened the monarchy and contributed to the revival of republicanism, which helped topple the Orleanist throne.

Mansel's book is generally well researched and written. But there are weaknesses. Having treated the subject chronologically, he tacks on a chapter on money and the court, which should have been integrated into the preceding chapters. There are occasional quotations that cannot be readily attributed. Finally, the index is somewhat incomplete. These criticisms aside, Mansel makes a useful contribution to the literature, and his work is to be commended.

JAMES K. KIESWETTER
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PADDY GRIFFITH. *Military Thought in the French Army, 1815–51*. (War, Armed Forces, and Society.) New York: Manchester University Press; distributed by St. Martin's Press, New York. 1989. Pp. viii, 236. \$65.00.

The contention of Paddy Griffith's well-documented and elegantly written work is that the French army of the Restoration and July Monarchy was far from the decadent force resting on its Napoleonic laurels frequently depicted after the debacle of 1870. On the contrary, after enumerating the army's successful campaigns in Spain in 1823 and in Greece, Antwerp, Rome, and Algeria, as well as its (eventual) mastery of urban warfare in France, Griffith chronicles the activities of a number of military writers and reformers whose work touched on virtually every aspect of military life, from strategy and tactics, through the technical branches and military schools, to the welfare of the soldiers in regimental schools and libraries. Griffith concludes that the French army was perfectly suited for its mission of fighting small wars and defending the social order against revolution. It was skilled and experienced and had developed military science to a point that earned it praise as the best army in Europe, at least after the Crimean War. For these reasons, Griffith believes that "France deserved a happier outcome" in 1870, because the advantage in efficiency in that war was only marginally in Prussia's favor: "In 1870–01 the victory went one hundred percent to the side which enjoyed only a marginal edge" (pp. 5, 168), he concludes.

Griffith provides a wealth of new and extremely interesting information about French military "thought" in this period. He certainly makes a convincing case that the French army was both tough and innovative, at least up to a point. As one might expect, his analysis of tactical thought is particularly good. In his eagerness to rehabilitate the reputation of the pre-1870 French army, however, the author sometimes underestimates its shortcomings. It had some notable failures in Algeria, which included the siege of Zaatcha in 1849. And, although the poor leadership of the Italian campaign of 1859 and the problems encountered by the army in Mexico lie beyond the scope of this work, these shortcomings were not manufactured by the army of the Second Empire. Nor did the origins and education of most officers, the rough-and-ready atmosphere of the army, its torpid life in drab French garrison towns or in the bleak hardships of Algeria encourage intellectual pursuits.

Despite these problems, Griffith demonstrates that there were reformers in the French army and that some of these men even achieved success in certain areas. What the army lacked was an organization capable of evaluating and systematizing ideas—the *dépôt de la guerre* was no Prussian general staff nor the *école d'état major* a *kriegsakademie*. As Griffith points out, the French continued to believe leadership to be a question of personal inspiration and rejected Clausewitz as irrelevant to French conditions. There was

certainly much original speculation by Ardant du Picq and others about battle as an interplay of moral forces. But battle was also about generalship and logistics, two categories in which the French army proved notably deficient in 1870. Nor did French army thought, like French military organization in this period, appear capable of expanding the confines of the small army mold. Clausewitz and the Prussian army speculated and organized on a scale that went beyond the limits of their narrow "mission," which provided the Prussian army with the framework for expansion under Bismarck and a margin of victory in 1870 greater than that which Griffith appears prepared to concede.

DOUGLAS PORCH
The Citadel

COLIN HEYWOOD. *Childhood in Nineteenth-Century France: Work, Health, and Education among the "Classes Populaires."* New York: Cambridge University Press. 1988. Pp. xii, 350. \$44.50.

Childhood may have been "discovered" among the European upper classes in the early modern era, as Philippe Ariès suggested in *L'Enfant et la vie familiale sous l'Ancien Régime* (1960), but the vast transformations that created childhood for peasant and working-class children came much later. In this book Colin Heywood examines that broader transformation of the lives of the mass of French children, the crucial stages of which, he argues, came only after the middle of the nineteenth century.

Heywood offers a balanced and, for the most part, well-supported assessment of the impact of industrialization and state intervention on the lives of children. He discusses the conflicting nineteenth-century views on such contested subjects as child labor and the moral character of the proletarian family, sorts through the competing political agendas that shaped the debates on those subjects, and assesses the evidence available to the historian interested in the conditions of children during the nineteenth century.

Heywood's overall argument is that the situation of many French children probably worsened during the early part of the nineteenth century and began gradually to improve only during the second half of the century. He refuses to locate the problems of overwork or poor health exclusively in urban industrial areas, and he emphasizes the poor and often-deteriorating conditions in the rural areas of France as labor demands on many rural families intensified. He suggests the range of different family economies that coexisted in different regions and different sectors during the transformation to industrial capitalism, and he connects those economies, both peasant and industrial, with particular patterns of child labor. The possibility for the transformation of the conditions of children was, in his view, ultimately rooted in the transformation of the circumstances of those family economies,

even if the interventions of state authorities were not without consequence.

Heywood has chosen a synthetic approach. He emphasizes important associations between the history of work and the history of education, subjects too often treated separately. His study is national in scope, although he employs local evidence on various aspects of childhood, which he has drawn from departmental and communal archives in different regions of France.

This account displays the strengths and weaknesses of its broad scope. As a synthesis, it offers the reader an introduction to the nature of French archival materials on the subjects of child labor, child health, schooling, and state intervention into the working and living conditions of children (including an especially interesting analysis of the records of state officials concerned with enforcement of child labor legislation). In the notes and bibliography, Heywood presents a broad, if somewhat spotty, introduction to secondary works on those subjects. The combination of national and local evidence, although often illuminating, seems unsystematic at times. Heywood makes an effort to place in the national context the nine departments and four municipalities from which he has drawn local archival evidence. Such placement would have helped in the interpretation of the local trends and variations he discusses.

Nevertheless, this book is a solid and timely one. A great number of studies concerning the history of the family and childhood in France have appeared since the publication of Ariès's book thirty years ago, and those works have long suggested the necessity for a revision of our understanding of the history of childhood. Heywood's book brings together the results of some of those studies and some original sources in a coherent and convincing survey.

MARY JO MAYNES
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JEAN-MARIE MOINE. *Les Barons du fer: Les Maîtres de forges en Lorraine du milieu du 19^e siècle aux années trente; Histoire sociale d'un patronat sidérurgique.* (Serpénoise.) Nancy: Presses Universitaires de Nancy. 1989. Pp. 563.

In 1605 a Lorraine monk in charge of his order's foundry died by falling into a rival's blast furnace while practicing industrial espionage. The century-old occupation of ironmaster (*maître de forge*) was built in this and other competitive ways that endured even when the industry reached its height around the turn of this century. Cutthroat pricing, political lobbying, exhausting work schedules, the creation of mutually suspect dynasties, and the recruitment of ever more technologically advanced talent all characterized the ironmasters' quest for greater material prosperity. Although the rise of iron and steel magnates occurred in all advanced industrial countries by the end of the nineteenth century, in France prosperity came to a region mutilated

by the Germans in 1871. A decade later, despite their truncated ranks, the Lorraine ironmasters began exploiting the rich Briey basin north of Nancy. Out of that exploitation, combined with the rapid modernization of iron and steel works in the Longwy area, grew what Jean-Marie Moine calls the "myth" of the "iron barons" of Lorraine.

According to this myth the iron families then proceeded to close off the region to newcomers, despoil it to their advantage, and, as a result, set the stage for the region's late twentieth-century productive collapse. Moine's meticulous social history investigates these intertwined allegations by exploring the inner workings of the ironmasters' world, particularly as it operated after 1870. He finds tight family networks and even dynasties. Yet these families welcomed talented young blood—especially engineers. They forced their own sons to endure long apprenticeships and technical training and even witnessed the founding of new dynasties, at least until the eve of World War I. Moine has a sharp appreciation for the varied occupational demands ranging from technical knowledge of minerals, other raw materials, and metallurgical processes to commercial abilities spanning regional, national, and international markets. The adept entrepreneur had to calculate the need for new products, lobby for tariffs and adequate transportation, and understand the most complex mechanical or staffing aspects of factory life. Whereas other historians have romanticized the Ruhr magnates as the quintessential industrialists to the detriment of the Lorrainers, Moine points to the current unemployment suffered in German metallurgy. As for nationalist charges that Lorrainers were pro-German in World War I or collaborators in World War II, Moine produces rousing evidence that for the most part they were as nationalist as the best, that they collaborated far less than the run-of-the-mill French industrialist, and that they participated in the Resistance far more. Vicious anti-Semites like Marcel Paul existed but were oddities, albeit dangerous ones.

Where then did the myths about the iron barons come from? Here Moine breaks through the confines of French social history. The ironmasters, he maintains, were themselves mythmakers, creating elaborate rituals such as anniversary banquets for their employees or regal funeral processions for one another. As a result they converted their financial assets into cultural power. Exploring the marital and ceremonial patterns of the Lorraine bourgeoisie, Moine shows how they produced a cultural image of solidarity and potency to overcome their mutual rivalries, insecurities, and real industrial difficulties. They hungered for this kind of cultural production as much as material production. For instance, the ironmasters constantly and usually unsuccessfully strove to control the professional, regional, and even national press as a way of further reproducing their image. For all of the money that they poured into such ventures, Moine suggests that the myth ultimately became something twisted, deformed, and hardly flattering. But here the story ends. Rather

than examine the other forces at work to refashion the iron barons' mythologizing, Moine returns us to the base of all social history—that myth is never real and that people enmeshed in that solid thing called "class" are. Only in leaving the reader wondering about the conflicting forces in the mythmaking process that produced the reductive and savage portrait of the predatory, fascist ironmaster does Moine seem at all remiss. Otherwise this study is social history at its most engaging.

BONNIE G. SMITH
Rutgers University

MARY LYNN STEWART. *Women, Work, and the French State: Labor Protection and Social Patriarchy, 1879–1919*. Buffalo: McGill-Queen's University Press. 1989. Pp. ix, 227. \$34.95.

Mary Lynn Stewart's diligently researched study of protective labor legislation in the early Third Republic makes an original case. Stewart believes that legislation that purported to protect and defend the interests of working women in fact had two untoward effects. Legislation that effectively prevented women from working at most skilled industrial jobs created a "dual labor market." This system, in turn, supported what Stewart calls the "patriarchal family." Women, in other words, were treated in the industrial working world in a manner that mirrored their secondary status within the working-class family where they continued throughout the period to bear the burdens of child rearing and housework.

To make her case, Stewart has researched every sort of official document, including ministerial reports, parliamentary debates, official reports from regions and localities (primarily including the departments of the Meurthe-et-Moselle, the Nord, the Rhône, the Seine-Maritime, and the Vosges), and various government publications. The book examines the debates and resulting legislation concerning working hours, night work, health and safety, and maternity leave. In each case, Stewart shows that the effects of legislation were, in the main, deleterious to working women's advancement toward the emancipation that might have resulted from women's economic independence from men. In many instances, moreover, particularly when legislation restricted women's access to night work, women workers themselves recognized the threat to their existence and protested, although, Stewart suggests, it was to little avail.

The book provides a good deal of information hitherto available only in fragmented form. Stewart has charted the passage of labor legislation with care and presents the results in readable tables. Her portraits of the legislator and industrialist Richard Waddington and the reformer Gustave Dron are interestingly and thoroughly drawn, and her exegeses of various governmental commissions' reports are intelligible. The book's conclusions about both the process of legislating

labor reforms and the ineffectiveness of much of that legislation are reliable. As the author shows, reformers relied on a variety of specious arguments about the protection of women's health, women's fertility, and the working class family as they worked toward legislation that did little to accomplish their articulated goals and much to cast women into permanent economic dependence. Most labor legislation remained unenforced, of course, and, given Stewart's assessment of its effects, this was not entirely a bad thing. Stewart reveals how ineffectual the inspection service remained throughout the period because of inadequate staffing and a lack of women or working-class inspectors who might have had more success eliciting complaints from intimidated women workers.

The weaknesses of this short book stem from the virtual absence of research in those materials that offer historians at least a glimpse of the views of workers themselves: workers' newspapers, the sparse letters of labor union militants (including female militants), the multitude of police spies' reports, and the rare—but extant—reports of investigative journalists concerned about documenting the working lives of women industrial workers. Although the book offers a clear view from above of the process through which labor laws were passed in France, one has almost no view of the reactions from below to such legislation. Instead, Stewart offers unsupported assertions about the results of legislation that she assumes should have been the case. She remarks, for example, that one effect of labor laws was to encourage industrialists to increase discipline of their work force. This, in turn, meant that women “lost what control they had over the pace of their labor and some of the pleasures of workplace sociability” (p. 202). Stewart is more tentative and therefore more reliable in her claims about the likely domestic effects of labor laws: “in the home . . . we do not know if they did more housework or exercised an increased moral authority” (p. 202). Stewart also makes claims that run counter to abundant recent literature: “most women did not complain about working conditions” because, in her view, they were “insecure in over-crowded occupations” and “without unions to voice their grievances” (p. 159). Of course, many recent monographs have shown that insecurity was not the exclusive situation of women workers and that few male workers had unions. Moreover, the constant strikes of the early Third Republic suggest that many workers of both sexes objected to laws restricting the work of women and children as well as to other changes Stewart documents. As many historians have argued, membership in organizations—whether trade unions or socialist parties—was not necessarily connected to labor militancy.

Still, this book summarizes with clarity the story of protective labor legislation, and at the same time it argues against much of women's history's “received wisdom.” Moreover, Stewart's briefly outlined view that the patriarchal family was bolstered by laws that effectively divided the industrial work force by gender is a provocative one, and it is one that should stimulate

both exploration and discussion by social historians of France.

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WILLIAM D. IRVINE. *The Boulanger Affair Reconsidered: Royalism, Boulangism, and the Origins of the Radical Right in France*. New York: Oxford University Press. 1989. Pp. viii, 239. \$34.95.

Few topics have drawn more attention in French political history than the shift from the traditional to the radical Right. The shift usually is considered to have taken place in the late nineteenth century, and the Boulanger affair has been judged pivotal in the transition. Although William D. Irvine does not deny the urban, middle-class, radical Republican affinities of Boulangism (those attributes of the modern Right that distinguish it from the peasant-based, notable-dominated political system of the traditional Right), he chooses in this “reconsideration” to view Boulangism more as an expression of the traditional than of the modern Right. The book in fact is not as much about Boulangism as it is about French royalism. This emphasis is clearly expressed in the organization. Irvine devotes the first two (of six) chapters to Royalists and the Third Republic and Royalist political organization. Boulangism is not the focus at all. Thereafter, Irvine develops his thesis that Boulangism was really just an attempt, similar to that of the Royalists of the Weimar Republic, to save the Royalists politically by hitching themselves to a popular base (see chap. 3, “Boulanger: Savior of the Monarchy,” and chap. 4, “Royalists and Boulangists”).

This is a book with a thesis, and Irvine's thesis, as far as it goes, makes sense. The marriage between Royalists and Boulangists was expedient. In an era of mass politics, the Royalists needed the electoral skills of the Boulangists, even in the rural areas where the politicization of village life undermined respect for the social order on which Royalists relied for political power. The Boulangists, on the other hand, needed the Royalists, too, because, with a few spectacular exceptions, the Boulangists could not win in urban areas, and Royalists still had enough of a base in the countryside to make a difference when Bonapartists and Republicans confronted each other.

Yet there is something troubling about Irvine's penchant to reduce the discussion of the Boulanger affair primarily to electoral maneuvering. Boulangists and Royalists had very little in common. Irvine acknowledges as much when he notes that, whereas Boulangists could do the things necessary to win popular elections, the Royalists were temperamentally ill at ease in a democracy. The critical point in the story, then, is the inability of Royalists to adapt themselves to mass politics, for this failure led to the demise of royalism as an electoral force in the 1890s. The book's chief shortcoming, therefore, is that it deals too perfunctorily with

royalism to explain why it could not adapt to mass politics. The Royalist vision of life, politics, and society made Royalists about as incompatible with the Boulangists as with the Republicans.

Despite the benefit that Boulangists received from electoral alliance with Royalists, which Irvine demonstrates, the alliance between Boulangism and Royalism could go nowhere: it was a political non sequitur and consequently of minor importance. The traditional interpretation of Boulangism, therefore, seems right: Boulangists should be remembered not as the political allies of a moribund traditional Right that nothing could save from extinction but as forerunners of the antidemocratic, mass-action, twentieth-century Right.

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JOAN UNGERSMA HALPERIN. *Félix Fénéon: Aesthete and Anarchist in Fin-de-Siècle Paris*. Foreword by GERMAINE BREE. New Haven: Yale University Press. 1988. Pp. xv, 425. \$35.00.

A book that is a labor of love and that lights up a hitherto obscure portion of *fin-de-siècle* French life deserves an enthusiastic welcome. Joan Ungersma Halperin's beautifully illustrated biography of Félix Fénéon is the product of twenty-five years of devoted and patient scholarship. In this book, along with her edition of Fénéon's *Oeuvres plus que complètes* (1970), she tells us more than we might have expected to know about a figure who hid much of himself behind the cool façade of the top-hatted dandy and aesthete.

Fénéon is best known today for his early championing of such men as Arthur Rimbaud, Francis Poictevin, and Jules Laforgue; for the art criticism that helped establish Neo-Impressionism as a separate and significant school, explaining its "objective" techniques while evoking and defending the independent personality of its major artists; for his stewardship of the journal *La Revue blanche*; for his *Nouvelles en trois lignes*, the pithy and often startlingly phrased newspaper accounts of current events that have been cited as predecessors of "minimal" story writing; and for the exhibitions and sales of contemporary paintings he organized at the Bernheim-Jeune gallery after 1906. Halperin writes perceptively about all of these things, but she also provides revelations about Fénéon's remarkable relations with women and about his anarchism.

That Fénéon spoke out in favor of anarchist propaganda by both word and deed is well known. But Halperin has concluded that he also planted the bomb that injured several people and cost Laurent Tailhade an eye in the Foyot restaurant in April 1894, a crime for which he was tried and acquitted. (The verdict owed something to Fénéon's witty and deflating responses to the prosecutor's questions at the trial.) Halperin's argument is far from iron clad; it is based largely on what one friend of Fénéon was said to have

told some others. But, given the explosive chemicals and detonators found in Fénéon's office at the War Ministry and his close relations with the devoted terrorist Emile Henry, Halperin may well be right. We shall never know for sure, but her position is a breath of fresh air in face of the haughty chorus of shocked denial that still echoes in the defenses by Stéphane Mallarmé and other of Fénéon's literary friends.

Whether or not Halperin's attribution of the act to him is correct, her way of dealing with it raises many questions. The details she supplies about his arrival at the café, his temporary departure from it to allow more clientele to arrive, his climb to the top of a bus afterwards all seem to be simply invented—a bit of literary panache that might have pleased her subject but that seems out of place in a work so scrupulously attentive to evidence in other respects. Halperin's attitude toward terrorist acts is indulgent. She never considers whether any motives other than revulsion against an unjust society may have motivated even so problematic a figure as Henry, who was happy to injure modest and ordinary people because they did not share his kind of commitment to violent change. She admits that terrorism may be counterproductive because it "hardens the hearts of the powerful" (p. 275), but she does not ask about its effects on the innocent. (Like Henry, does she believe there are none? I doubt it, but her position is far from clear in the book.) Her general view of *fin-de-siècle* French society as oppressive and unsympathetic to the avant-garde has a one-dimensional quality that accords well with her treatment of terrorism but is certainly simplistic. We must also ask different questions about the character of a guilty Fénéon than we would about a Fénéon falsely accused. Can we dissassociate his willingness to injure others from the qualities noted by those contemporaries who saw him as indifferent to, even contemptuous of, those around him? And what about the police? Have we the right to feel superior to them, even granted that the conspiracy to which they believed Fénéon belonged was largely imaginary, if they had the right man all the time? For all of its virtues, Halperin's book only adds to one's sense that the relations between aesthetes and politics were murky and troubling in the last century, as they remain in our own.

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HELENA LEWIS. *The Politics of Surrealism*. New York: Paragon House. 1988. Pp. xi, 229. Cloth \$25.95, paper \$12.95.

The place of the French Surrealists and their non-French allies in the canon of early twentieth-century avant-garde culture has by now been securely established. André Breton's function as an impresario of culture and organizer of other people's talents is also clear. But can French Surrealism be understood as a purely cultural phenomenon? Helena Lewis thinks that

it cannot, and it is to her credit that she has decided to approach the problem of the "politics of Surrealism" within the larger context of the politics of the avant-garde in early twentieth-century Europe. Stating boldly her conviction that, in this century, art and politics have become inseparable, she claims that of all avant-garde groups, the Surrealists "developed the most sustained and coherent political beliefs" (p. ix).

To take Surrealist politics seriously is to swim against the current of the prevailing historiography. Lewis is quick to acknowledge that Surrealism had its origin in Dadaist groups that were anarchist in their social thought and political inclinations. "No more socialists, no more Bolsheviks, no more aristocrats, no more armaments, no more politics, no more countries" (p. 5), the Dadaist poet Aragon wrote in 1920. By 1925, though, under the charismatic leadership of Breton, some former Dadaists, who now called themselves Surrealists, were moving toward an acceptance of the need for a proletarian revolution. Why the change? Lewis believes that it was because the Surrealists were running out of purely cultural targets on which to vent their spleen and also because Breton had become convinced that the precondition of a revolution of the spirit was a social revolution similar, if not identical, to the one that had recently occurred in Russia.

Under the conditions prevailing in France in 1925, the decision to pursue a revolutionary politics necessarily raised the question of an alliance with the French Communist party (PCF) and the Comintern, to which the party was now at last securely subordinated. The trouble was that the leadership of the PCF and the Comintern had little sympathy with the libertarian goals and artistic experiments of the Surrealists. The liberation of the unconscious from the tyranny of reason through automatic writing and seances, the blurring of the distinction between dream and reality, and a preoccupation with suicide and unconventional sexual behavior ran against the deepest convictions of a party that had as its chief goals the dictatorship of the proletariat, the triumph of dialectical materialism over subjective idealism, and the imposition of Communist order on a society characterized by unchecked individualism. The result could only be a highly unstable liaison, and by 1934 Breton and those faithful to him had declared open warfare against the PCF, the Stalinist regime, and its literary doctrine of socialist realism.

In search of the coherence that Lewis promises to show in Surrealist politics, I found myself hesitating before several possibilities that are either implicit or explicit in her account. One, of course, is ideology. But, when scrutinized, the various definitions that Lewis gives of the Surrealists' political convictions seem either unconvincing—as when she ascribes to them, for example, an original synthesis of Karl Marx, Sigmund Freud, and Arthur Rimbaud—or so general as to be applicable to countless other groups—as when, for instance, she comments on their unswerving dedication to liberty. A cynic might say that the only thread that runs throughout the book consists of Breton's indefat-

igable efforts to impose his ideas on intellectuals with Bohemian and left-wing inclinations. But what strikes me as the real principle of continuity, and the precondition of Breton's remarkable success in attracting wave after wave of disciples over a period of twenty years, is that persistent and ill-fated attempt of intellectuals in early twentieth-century Europe to combine their hate for the class and culture that produced them with an unquestioned faith in the necessity and desirability of a proletarian revolution conceived along Marxist lines.

Lewis is certainly right to conclude that Breton experienced more deeply and over a longer period of time than most of his contemporaries the problems involved in fusing a cultural and social revolution. But what Lewis takes to be one of Breton's strengths—his refusal to break with the idea of a left-wing proletarian revolution long after he had been isolated from the party that could claim to represent the proletariat—could just as easily be considered a serious failing. Unlike Herbert Marcuse, with whom Breton shared the desire to combine Marx and Freud, Breton never seems to have considered the possibility that there might be an insuperable contradiction between proletarian revolution and the type of cultural revolution to which he dedicated his life.

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NOËLLE GEROME and DANIELLE TARTAKOWSKY. *La Fête de l'Humanité: Culture communiste, culture populaire*. Paris: Messidor/Editions Sociales. 1988. Pp. 340. 350 fr.

Every September the French Communist newspaper *L'Humanité* sponsors a huge fair in a Paris suburb in order to raise funds. The *Fête de l'Humanité* quickly outgrew its original purpose and became rooted in popular tradition in the Communist political subculture in France. Local federations of the party established booths and sold regional gastronomical specialties; families and friends looked forward to the fair as a prime opportunity for the expression of working-class sociability; and Communist leaders used it, as the principal political event of the *rentrée*, to announce variations or changes in the party line. In the 1950s and 1960s, corporations began to sponsor exhibitions at the fête, ever in search of advertising, and a distinct aura of the new youth culture invaded, as political chansons were pushed aside in favor of rock music and here and there one could pick up the distinct aroma of marijuana. In all of this the original aim of left-wing solidarity, both national and international, was never lost. Political enemies from Léon Blum to Charles de Gaulle were symbolically conquered, their faces serving as targets on dart boards and the like; funds were raised for folk heroes as diverse as Henri Martin (an early opponent of the Indochina war) and Nelson Mandela; and the virtues of the Soviet Union, the East-bloc nations, China, and later Cuba were extolled

in popular exhibitions and political propaganda. Other political movements, such as the Gaullist Rassemblement du Peuple Français, briefly experimented with similar kinds of popular events, but none were successful in the long run. Today the *Fête de l'Humanité* enjoys the distinction of being the only event of its kind, at once expressive of Communist values yet a permanent part of the broader national political landscape, getting considerable coverage in the press and demonstrating the party's successful integration into French life.

Danielle Tartakowsky and Noëlle Gerome have attempted to treat the fête with the techniques of historical and cultural anthropology. Tartakowsky provides a historical survey, demonstrating similarities with working-class and popular manifestations of the nineteenth century, narrating the growth and development of the fête since its origins in the 1930s, and rather dryly recounting the series of moves from suburb to suburb and the increasing numbers of exhibitors, both national and corporate, who have elected to participate. Gerome attempts a more ethnological treatment, examining popular symbolism, analyzing quotations from Communist militants who regularly attend, and stressing the fête as a family event and at the same time an expression of Communist political culture and values. Both insist that the two or three days of combined mass political meeting, rock concert, and county fair, with attendant sixty-nine varieties of *merguez* (porkless sausage of North African origins) and *frites*, in some fashion symbolically prefigures the achievement of the future social order that is the goal of the party's political ideology and program, namely, a society that is imbued with the values of freedom, equality, and fraternity.

One may accept the authors' argument that there is a good deal of spontaneous mass enthusiasm that has gone into the fête's success, and clearly there are links with the popular movements of the nineteenth and earlier centuries. But it is doubtful that techniques developed to explain the *journées* of the French revolution or the manifestations of Rabelais' day can be used successfully for what is, in the end, a contrived event carefully tailored to suit the needs and purposes of a centralized, Leninist, political culture. Indeed, the authors cannot fail to remind us of this, even as they gloss over the conscious promotion of the personality cults of Stalin, Maurice Thorez, and even, to a degree, Georges Marchais, which once made up so much of the fête's content. Communist political history, as it appears in this history of the fête, remains a glorious tale of the struggle for peace, social justice, and freedom from oppression, unsullied by political and social isolation, home-grown purge trials, the mistaking of friends for enemies, and solidarity with Stalinist crimes, which have made former Communists one of the largest numerical groupings in France. One wonders after reading through this volume, which has the virtue of treating the history of an event covered nowhere else in the historical literature, whether one has been exposed to a serious effort at historical understanding

or a political propaganda tract. Perhaps (alas) from the authors' viewpoint, the book is both.

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SCOTT MCCONNELL. *Leftward Journey: The Education of Vietnamese Students in France, 1919-1939*. New Brunswick, N.J.: Transaction. 1989. Pp. xix, 195. \$34.95.

This is a monograph that raises important issues in the tortured history of French relations with Vietnam. Scott McConnell is to be commended for a contribution to a relatively sparse literature on the interwar years, but the book leaves the reader dissatisfied with the author's attempt to explain the radicalization of Vietnamese student nationalism.

McConnell's major contribution is to treat critically the dilemma of French liberal imperialism, the gap between the theory and practice of French colonists' self-justifying "civilizing mission," particularly in demonstrating the limitations of reforms proposed by Albert Sarraut, who is often seen as an exemplar of a new and more enlightened approach. French attitudes remained paternalistic, and French policy, no matter how liberal in conception or intent, remained at odds with Vietnamese nationalist aspirations. The major failure was in education, an important test of liberal theory and practice. Rather than offering enlightenment and preparing students for more responsible roles in Vietnam, education became a means of control, at most a safety valve for a limited elite or, in the eyes of conservative critics, a risky experiment that introduced dangerous ideas. Liberalism was not for export to the colonies.

The inherent contradictions in the program and policies of liberal imperialism are sharply delineated, but, when McConnell tries to explain the failure of liberalism to take hold among Vietnamese students, the result is much less satisfactory. By making a case for the decade of the 1920s as the turning point in the radicalization of a generation of Vietnamese students, the author underestimates the importance of the generation represented by Phan Boi Chau and those who adopted political terrorism after 1900 as a bridge between the traditionalist opponents of French rule of the 1880s and 1890s and the radicals of the 1920s and 1930s.

A more serious objection arises from the author's attempt to explain communism's appeal to young Vietnamese nationalists. French repression certainly disillusioned many Vietnamese with French claims to be inspired by the rights of man in colonial policies. But neither the radicalization of Vietnamese students who went to France in the 1920s nor the turn toward Marxism or Trotskyism in the 1930s can be explained solely by French actions or the shortcomings of French educational policy. The author speculates about the reasons for these developments, offering certain Viet-

namese cultural patterns, such as Confucianism, as possible explanations. Evidence for these assertions is derivative from the work of others. Even the argument that the noncommunist Left in France shares the blame for French policy by its failure to respond to Vietnamese demands can be no more than asserted. To find the Vietnamese roots for nationalist radicalism, the reader must look elsewhere, notably to the works of David Marr, Barton Woodside, William Duiker, and those scholars who have used Vietnamese sources to gain insight into the indigenous culture.

A narrowly applied French policy is one part of the explanation for the failure of a "liberal-democratic" alternative in Vietnam, but this analysis tells us more about French cultural perceptions and assumptions than it does about the response of the Vietnamese students. Had the author limited himself to this issue and pressed further into the reasons for the failure of French understanding of Vietnam and the shortcomings of liberal imperialism, the book would have been more satisfying.

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JOAN SHERWOOD. *Poverty in Eighteenth-Century Spain: The Women and Children of the Inclusa*. Buffalo, N.Y.: University of Toronto Press. 1988. Pp. xv, 239. \$38.50.

The theme of poverty is one that has enormous resonance in Spanish history, touching as it does on so much of the art and literature of that country, on the ideology of its great church, and, not least, on the tardy development of capitalism in the peninsula. It is with one form of poverty, that surrounding the abandoned infant, that Joan Sherwood's fine study is concerned. The *Inclusa* of the subtitle was the foundling home in Madrid set up in 1572 by a charitable guild, around the same time as similar organizations in other Spanish cities, of which there have been some good studies in recent years. The problem of the foundling can best be seen against the demographic background of Spain's capital city, ably charted by María Carbajo Isla. The population edged steadily forward over the eighteenth century from one hundred thirty thousand to one hundred ninety thousand individuals, in spite of generally deteriorating economic circumstances that held back marriage and fertility. The growth seems the result of the misery of a rural exodus. It is against this background that Sherwood's book is set. The *Inclusa* accounted for about 10 percent of baptisms in Madrid until the 1790s and about 15 percent thereafter. Thanks to some deft and on the whole convincing calculations, Sherwood infers that, although a majority of these children may have been illegitimate in the early part of the century—victims of Spain's rigid code of honor—after 1750 something like half may have been simply abandoned by parents too poor to feed them. One may feel that the distinction between legit-

imate and illegitimate at this social level is somewhat academic anyway and agree with Sherwood that the problem of foundlings was the lack of resources of parents.

The author is fundamentally concerned with the running of the institution and with the care of the inmates. Never well funded, the *Inclusa* faced mounting problems in an inflationary and increasingly pauperized society. Despite the concern of enlightened thinkers in Spain, as elsewhere in Europe with more rational and efficient treatment of poverty, the increasing diversion of funds from the church in the 1790s by the government seems to have weakened the one organization capable of investing in poor relief. Although Sherwood reminds us of the attempts, especially under Manuel de Godoy, to admit foundlings to full civil status, my impression is that the treasury had more pressing commitments, namely war, for its (and the church's) money in this period. In a long and interesting section, the author explores what we know about the social background of wet nurses, suggesting that as many came from Madrid itself as from the countryside (unusual in the European context, as may be the urban origin in Spain of the overwhelming majority of illegitimates). Low pay, inadequate control, and poor hygiene of nurses contributed to the horrendous mortality characteristic of the system. It is with this phenomenon that some of the author's fullest and most interesting discussion is concerned. A government enquiry of 1790 reported that about two-thirds to three-quarters of foundlings admitted to the various refuges in Spain died before age six or seven. The record of the Madrid *Inclusa* seems to have been among the worst—paradoxically, perhaps, in view of an increasing concern with the bodies rather than with the souls of the infants and a greater role for the medical staff in the 1790s. Although Spain had a good pediatric school, it seems to have operated largely within an antique framework of medical theory, wedded to purgings and bleedings as a panacea, while the experiments with feeding infants by hand seem to have been an inadequate substitute for better pay and control of the wet nurses.

One puts down Sherwood's book feeling enriched by a sympathetic portrayal of human endeavor and failure. Some of the terrain may be familiar, from the work of Spanish historians on other foundling homes, but it is good to have a discussion of this major problem available now in English. One may query one or two of the author's statements—for example, that grain yields were generally as low as three to one in eighteenth-century Castile (p. 59) or the inference that the *espolios y vacantes* were anything other than temporary revenues from vacant ecclesiastical benefices (p. 183). And there are an irritating number of misprints. But what is refreshing about the book is its basic humanity, its attempt to give the babies and the wet nurses faces, rescuing them from the limbo of statistics. One recalls the description of how straw was used to make mats, brooms, and bowls, "in short, almost any item for the

kitchen that did not have to hold water or go over the fire" (p. 12). This is an evocative study indeed of the world we have lost.

JAMES CASEY
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J. P. FUSI. *Franco: A Biography*. Translated by FELIPE FERNANDES-ARMESTO. Foreword by RAYMOND CARR. New York: Harper and Row. 1987. Pp. xii, 202. \$25.00.

J. P. Fusi offers a concise summary of the history of Francisco Franco's regime from its foundation in 1936 until the death of its founder in 1975. Fusi's account of the first two decades focuses on the interplay of domestic and international events. He examines Franco's relations with the Axis and the Allies during World War II, the censure of the regime by the United Nations and the withdrawal of ambassadors in the immediate aftermath of the war, and the gradual turnaround of the international climate because of the cold war. By the time of Spain's admission into the United Nations in 1955, Franco had successfully put behind him the period in which international events threatened his regime's stability. "At last I have won the Spanish Civil War," Fusi quotes Franco as saying after the signing of Spain's first agreement with the United States in 1953 (p. 80).

The second two decades of the regime's life were marked by rapid economic development and the ultimately unsuccessful attempt to devise a set of institutions that would ensure the regime's survival beyond the life of its founder. Fusi wisely dedicates little space to the relatively familiar story of the Spanish economic "miracle," preferring to examine the political evolution of Spain and of the Franco regime during the period of rapid economic development.

One of the most interesting aspects of the book is Fusi's analysis of the relations between Franco, the monarchists, and the two principal contenders for the Spanish throne, Don Juan de Borbón, who was the son of King Alfonso XIII, and Don Juan Carlos, Don Juan's eldest son. Franco probably discarded Don Juan as a viable contender for the throne as early as 1942 or 1943 and almost certainly no later than 1945. In later years Don Juan softened somewhat his opposition to the regime, but Franco continued to consider him an opponent tainted by liberalism. Nonetheless, Franco early fastened his attention on Don Juan's eldest son, Don Juan Carlos, as a potential successor and groomed him for the position. Not until 1969, however, did he actually nominate Don Juan Carlos as his successor, and even after the nomination he continued to resist pressure to hand over power during his own lifetime.

The book's title is somewhat deceptive. What Fusi has in fact written is a brief political-biographical essay. He offers a shrewd and well-balanced appraisal of Franco's political career but little insight into his personality. Beyond noting Franco's stoicism, his aversion to liberal ideas, and his adherence to traditional Span-

ish and Catholic values, Fusi makes no attempt to bring to life a personality. From the political point of view, Fusi sees Franco not as a fascist ideologue but as a soldier hostile to liberalism and democracy who created an authoritarian regime that he hoped would preserve unity, order, and the values that he viewed as threatened by communism and freemasonry.

Fusi states that he has attempted to offer a "historical interpretation," avoiding "eulogy and calumny alike" (p. xiii). Although Fusi condemns Franco's authoritarian policies and considers the regime a failure because of its rejection of liberal democratic principles, he recognizes that Franco enjoyed lasting support among wide sectors of Spanish society and acknowledges his success in achieving stability and overseeing a remarkable transformation of Spain's economy and society, a transformation that he rightly sees as ultimately undermining the state Franco hoped to preserve.

I disagree with a number of Fusi's assessments, especially his analysis of the role of the Catholic church, of whose positions and doctrines he seems to have little understanding, but on balance Fusi presents an intelligent survey of the political life of Spain during the Franco years.

JOHN F. COVERDALE
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SEBASTIAN BALFOUR. *Dictatorship, Workers, and the City: Labour in Greater Barcelona since 1939*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xii, 288. \$59.00

Barcelona, wrote Friedrich Engels, was the city that had seen more barricades than any other. And that was before the conflicts of the early twentieth century. Despite the frequency of such eye-catching social conflict, the historiography of the Barcelona working class for the period before the Civil War is underdeveloped. For the period since 1939, the situation is, predictably, much worse. That fact makes Sebastian Balfour's excellent book even more welcome.

Balfour combines an analysis of urban development and social change in greater Barcelona with a fine-grained reconstruction of the history of the labor movement since 1939. He takes us not only from district to district within Barcelona but also from factory to factory, while never permitting the hard-won details to obscure his overall analysis. His research is formidable. He interviewed sixty labor activists, read a wide range of the clandestine labor press, and consulted the archives of the civil government of Barcelona, which contain extremely valuable reports from the secret police, the Civil Guard, and the state unions.

Balfour does not just narrate the history of local labor movements; he also relates local developments to broader questions. Indeed, his central concern is to determine why a working class that had played such a central role in the transition to democracy became the "poor relation" of the new regime. Basically he argues

that worker militancy was shaped by the role of the Francoist state, especially the legalization of collective bargaining through the official unions in 1961. The clandestine labor movement embodied in the Workers' Commissions emerged out of the bargaining process, and its strength lay precisely in its ability to voice shop-floor concerns.

This source of strength also produced a weakness; differences in structure from one locality or one industry to another produced a movement that was highly fragmented and lacked any unifying bond. Ideology, which had provided cohesiveness in the past, did not have a significant role, and the part it did play was a negative one, as the question of the identification of the Workers' Commissions with the Communist party demonstrated. Firmly anchored in immediate economic demands and lacking any political project beyond the attainment of basic democratic rights, unions yielded to political parties in the negotiations over political change. The power of the streets was effectively hijacked by opposition politicians, especially the Socialists, who then failed to provide the unions with a strong institutional position in the new system.

One question that immediately comes to mind when dealing with Barcelona is the fate of the Confederación Nacional del Trabajo (CNT). Balfour argues that anarcho-syndicalism was ill-suited to the conditions imposed by Francoism, that older anarchists were out of touch with the realities of a consumer society, and that the CNT's refusal to work within the state unions doomed it to disappearance. Certainly one great strength of the Workers' Commissions was the decision to work within the official union structure. As Balfour makes clear, those unions were not rejected as an unwelcome imposition by a hostile state; rather, they played an important role in the lives of Spanish workers. That the Franco years were not a caesura but a crucial formative period in Spanish life is too often ignored.

This book offers a readable introduction for anyone interested in contemporary Spanish social and labor history while rewarding specialists with its wealth of detail and interpretive sophistication.

ADRIAN SHUBERT
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D. L. RABY. *Fascism and Resistance in Portugal: Communists, Liberals, and Military Dissidents in the Opposition to Salazar, 1941-1974*. Manchester: Manchester University Press. 1988. Pp. ix, 288. \$49.95.

This excellent study helps us understand why the Salazar regime lasted longer than any other European dictatorship. D. L. Raby argues against the theory that the regime's longevity derived from the passive support of the Portuguese people and shows instead that resistance to the regime grew steadily from the 1940s on. In relating the story of Communist, Populist, and

Liberal opposition, the author argues that the regime endured both because of its enemies' errors and because institutionalized dictatorships are extremely hard to dislodge in the absence of foreign intervention.

Raby's second point is not well developed in the text, but her argument about the regime's enemies is very well sustained. Two chapters on the Portuguese Communist party and two chapters on military and civilian populism are supplemented by more general discussions of opposition in the period from 1941 to 1957 and 1962 to the revolution in 1974. Through careful use of interviews, party documents, and newspapers, Raby gives us both a beautifully written chronicle of how men such as Alvaro Cunhal, Humberto Delgado, and Henrique Galvão acted and a convincing analysis of why they acted as they did. We learn how these and other opposition leaders saw the world, their compatriots, and one another.

There is probably no book in any language that gives us such a vivid and balanced picture of the workings of the prerevolutionary Portuguese Communist party. The party's role in the revolution and its internal conflicts today are greatly illuminated by the historical materials presented here.

One always feels obliged to offer some point of criticism on these occasions—even for work as fine as this—but my only disappointment with this study is its brevity. Raby teaches us how and why the opposition acted as it did, but she never fully explains why the Portuguese people failed to join opposition groups in greater number. We have an excellent study of opposition politics from above but are left with questions about what went on below—in the hearts and minds of the people whom the opposition leaders were trying to lead. Why, for example, was there not more mass mobilization during the Caetano regime (1968-74) when coercion was clearly easing? The book devotes fewer than fifteen pages to this period and thus leaves us uncertain.

This criticism aside, Raby's book is worthy of an audience extending well beyond the small community of scholars studying Portugal. Because of recent attempts to change regimes in Latin America, Eastern Europe, and Asia, the social sciences are now rife with debates about the appropriate roles and tactics for opponents of dictatorship. Raby's work brings new material and a sophisticated analysis to timely issues as she cautions that "opposition unity is never easy to achieve," but "hesitation is fatal" (pp. 267-68).

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FRANZ IRSIGLER and ARNOLD LASSOTTA. *Bettler und Garkler, Dirnen und Henker: Aussenseiter in einer mittelalterlichen Stadt; Köln 1300-1600*. Paperback edition. Munich: Deutsches Taschenbuch. 1989. Pp. 319. DM 16.80.

In this richly illustrated paperback, Franz Irsigler and Arnold Lassotta, both well acquainted with the Cologne archives, offer an entertaining collection of anecdotes about social outsiders, peripheral people, and people of low or unusual status in preindustrial German towns. The book is more antiquarian than historical, written for a popular (presumably mostly German) audience. But, for that genre, it is solidly done, grounded in primary sources rather than romance.

The title is rather misleading because the authors touch on far more than four marginal groups: rascals and loafers as well as beggars; traveling musicians, buskers, and actors as well as jugglers; gypsies, fortune-tellers, and sorcerers as well as harlots; beadles, dog-catchers, dead animal collectors, and cesspool cleaners as well as hangmen—not to mention physicians, barber-surgeons, quacks, lepers, lunatics, and werewolves. And, although the title suggests a study of medieval Cologne, the authors have actually included examples and stories from the seventeenth century and from several other German towns.

The authors' method is impressionistic rather than statistical, using the sources for illustrations but not for making generalizations. We find out, for example, that lepers wore a long white cloak and carried a rattle while prostitutes had to wear red, at least a veil or kerchief if not a red dress. We see taboos in action. In 1484 an apprentice goldsmith who had served his full eight years still needed help from men in high places to get into the guild; other apprentices had discovered that he was an undesirable because he was the son of a surgeon. At a beheading in 1513, the victim's head accidentally rolled into the crowd. The cooper who tossed it back was immediately expelled from his guild because he had become dishonorable by doing executioner's work. Or we learn that Cologne opened a municipal house of ill repute in the 1520s when other towns were closing theirs. Moreover, unlike other towns, the prostitutes in Cologne's official house were the town's least desirable.

In short, the book is interesting, knowledgeable, and even valuable for some purposes but, despite the 856 footnotes, not addressed to scholars. The bibliography omits the monographs of American historians of Cologne such as John Freed and Paul Strait but includes, in French, Honoré de Balzac and Philippe Ariès. The basis for selecting the anecdotes is not always clear (why tell us that Cologne saw its first elephant in 1482?). Above all, there are few systematic attempts to answer questions historians might ask: for example, did the taboos come from a few basic principles or serve a practical function? Or why did Cologne municipalize prostitution so late?

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ANTHONY J. LA VOPA. *Grace, Talent, and Merit: Poor Students, Clerical Careers, and Professional Ideology in*

Eighteenth-Century Germany. New York: Cambridge University Press. 1988. Pp. viii, 412. \$54.50.

Whether the butt of satire, the object of pity, or the target of cameralist scorn, poor students were a visible lot in eighteenth-century Germany. Christian Wolff, Johann Winckelmann, Immanuel Kant, and Johann Fichte were only a few of the figures who emerged from their impoverished ranks to make a mark on German intellectual life. They are the focus of Anthony J. La Vopa's sensitive and nuanced study, which uses them as a vehicle for examining the broader themes of academic mobility and the rise of professional culture in eighteenth-century Germany.

La Vopa is concerned exclusively with Protestant territories where the Reformation had given rise to varied but limited sources of financial support for students from humble backgrounds. These could include artisan sons (peasant households were rarely represented), but more common were the sons of obscure rural pastors who hoped to pursue the career of their fathers. For those of humble background, clerical careers were understandably attractive. A pastor's tenure was generally secure, and his income, though usually modest, was relatively safe from the vicissitudes of the market. And the prestige of the German pastor, enhanced as it was by the blend of civil and spiritual authority that distinguished his office, was an added inducement enabling those from humble backgrounds to wield relative authority and influence.

But for most theology students the professional path to a parish was strewn with financial and emotional deprivation. Even if the student managed to complete his studies, competition for positions was intense, and he faced the prospect of working years as a poorly paid tutor or schoolmaster. The psychic toll was no less exacting; as was generally true of social and professional advancement in the Old Regime, a complex network of patronage governed the trajectory of a clerical career. His need to depend on patrons heightened the aspirant's financial vulnerability and could easily undermine the self-respect his hard-won academic credentials had given him. Nowhere were the emotional costs of academic mobility more painfully felt than in tutoring posts, which often represented the poor student's first sustained contact with upper-bourgeois or aristocratic households. Ill-prepared socially, subject at any time to arbitrary dismissal, forced in relations with employers to strike a delicate balance between obsequiousness and arrogance, those who survived the experience (Fichte, for example) often recalled it with bitterness.

But here La Vopa makes an important point, obvious but often obscured by the more vulgar variants of modernization theory. The Old Regime was not a "closed" society, and a considerable degree of mobility was possible within and between its corporate structures. With a university degree, proper patronage, and the requisite luck, students from humble backgrounds could and did rise to respected positions in the clergy,

university, or bureaucracy. This, in turn, raises the question addressed by La Vopa in the second half of his book: how was mobility sanctioned in a society where meritocratic ideals had yet to win the day? For, if social mobility existed *de facto* in the eighteenth century, it also coexisted with traditional ideological sanctions against advancement beyond one's own station.

Pietism was to play a crucial role in forging an ideological compromise between station (*Stand*) and vocation (*Beruf*). On the one hand, Pietists like August Hermann Francke championed a work ethic whose psychology of achievement legitimated academic advancement and social mobility. On the other hand, they also insisted on the individual's need to undergo a prolonged "conversion experience" (*Bekehrung*) through which the student acquired a passive trust in Providence and hence an acceptance of limits on his mobility. In this way, the Pietist ethos functioned both to inspire and to contain ambition.

The Pietist compromise provided the ideological wedge through which later, more secular ideals of professionalism could emerge. But, while arguments for talent over birth had prevailed by the 1770s, opinions varied sharply on precisely what constituted merit. Although egalitarian spokesmen like Friedrich Gedike favored a more socially neutral ideal, what ultimately won out was a model whose meritocratic language concealed more subtle justifications for educational exclusion. The neohumanist model of the university rejected traditional claims of birth. Yet its cult of genius, its contempt for utilitarian conceptions of talent, and its emphasis on the cultured personality created new criteria of exclusion that continued to restrict the promotion of talent from below in the nineteenth century.

It is a pity that La Vopa, who on occasion draws illuminating comparisons with France, says nothing about Catholic Germany. Given his salutary insistence on the continued significance of Pietism for eighteenth-century German culture, some comparisons with Catholic academic culture would have been especially enlightening. It is also curious that Christian Thomasius, who played a critical role in redefining the role of the academic in eighteenth-century German university life, is not discussed. But although this is not an easy book—some judicious pruning and paring could have improved its flow—it is a pioneering inquiry into the circuitous paths that led from Protestant concepts of vocation to modern ideals of professionalism. It has much to say not only about eighteenth-century Germany but also about how educational inequality was justified and institutionalized on the eve of the emergence of the modern university.

JAMES VAN HORN MELTON
Emory University

KARL A. SCHLEUNES. *Schooling and Society: The Politics of Education in Prussia and Bavaria, 1750–1900*. New York:

Berg; distributed by St. Martin's, New York. 1989. Pp. ix, 269. \$52.50.

The subtitle of Karl A. Schleunes's book reveals its content more accurately than the title. Although he indicates that his conclusions are about the effects of the schooling revolution that took place in the late eighteenth and nineteenth centuries, effects that he considers to have been serially or even simultaneously "modernizing, integrating, controlling, liberating" (p. 4), Schleunes does not analyze these effects. Instead, he traces over a century and a half the attitudes about and policies toward elementary education of the monarchs and officials of the two leading German states.

The greatest contribution that the book makes, especially for an English-speaking audience, is its chronicling of Bavarian educational developments. Of particular value are Schleunes's discussions of the abortive effort to secularize education undertaken by Bavarian education minister Franz Gresser in the late 1860s and of the failed attempt to separate school districts from parishes proposed by his successor Johann Lutz.

Schleunes also makes interesting observations about Prussia. He notes, for example, that the elementary schools admired by a number of French and American observers in the 1830s were not those envisioned by the Prussian reformers but those administered by their conservative—if not reactionary—successors. Elementary school teachers entered the period of German unification "as the scapegoats for 1848; they emerged . . . as the heroes credited with having made Königgrätz and Sedan possible" (p. 160). Yet, in the *Kulturkampf*, the efforts to Germanize Prussia's Polish citizens, and the fight against Social Democracy, the elementary schools did not succeed in their assigned tasks. As Schleunes states, "Schools alone do not a nation make" (p. 197).

Despite these useful insights, however, the book does not make a significant contribution to German educational history. Schleunes does not formulate very clear questions and fails to make explicit what he thinks his study adds to our understanding of the schooling revolution in Germany. His devotion to an almost exclusively narrative approach leaves little space for interpretation. The fact that Schleunes does not acknowledge the assistance of any experts in the field suggests the extent to which his work has been done in isolation from recent scholarly debates.

The most serious shortcoming of the book, however, is that the bibliography contains no works published since 1982. In the active field of German educational history, this is simply inexcusable. In addition, Schleunes neglects many works available in 1982 that could have enriched or corrected portions of his study, including the first volume of Peter Lundgreen's *Sozialgeschichte der deutschen Schule im Überblick* (1981), John Kulczycki's *School Strikes in Prussian Poland* (1981), and the dissertations of American scholars Mary Jo Maynes, James M. Olson, Katharine Kennedy, James Van Horn

Melton, and this reviewer. Berg Publishers should have required a revision of the manuscript that took into account more recent scholarship before they accepted it for publication.

JAMES C. ALBISETTI
University of Kentucky

PETER PARET. *Art as History: Episodes in the Culture and Politics of Nineteenth-Century Germany*. Princeton: Princeton University Press. 1988. Pp. vii, 227. \$25.00.

In 1987, Berlin celebrated its 750th anniversary. Major museum exhibitions highlighted the city's political and cultural history. Both East and West Berlin vied to refurbish old monuments and to claim the former Prussian capital's cultural legacy. Among the statues to regain a prominent place on East Berlin's Unter den Linden after years in storage was the equestrian figure of Frederick the Great. Recent reevaluations of Prussia's most famous monarch underscore Peter Paret's argument that periodic revivals of historical figures, events, and their artistic representations serve a contemporary purpose. The author's informative chapters illuminate the importance of literature, painting, poetry, and graphics in creating a national historical consciousness. Paret's examples include the famed Franz Kugler and Adolf Menzel collaboration on the *History of Frederick the Great* (1842), a full discussion comparing and analyzing Alfred Rethel's inspired series of woodcuts of 1849, *Another Dance of Death*, together with Menzel's unfinished but moving canvas of the central event in Berlin during the revolution, *The March Casualties Lying in State* (1848). Menzel's later representation of Wilhelm I's departure to the front during the Franco-Prussian War is described and compared with the several versions of Anton von Werner's polished paean to Prussian victory, *The Proclamation of the German Empire*.

Paret is the well-known author of previous works on Prussia's military during the Napoleonic epoch as well as a fine study of a modernist artists' organization at the end of the nineteenth century, the Berlin Secession. In this latest volume, he deftly demonstrates that the painters and poets were increasingly drawn to illustrate historical events in a fashion similar to that of a growing number of historians, such as Leopold von Ranke, who wished to describe history "*wie es eigentlich gewesen*." Paret's fine first chapter on the Kugler history of Frederick the Great, illustrated by Menzel, argues that the new historical consciousness affected a changed artistic sensibility, reflecting a mid-century liberal interpretation of Frederick's rule. During the second half of the century the *History* was immensely popular as were countless plays dedicated to Frederick the Great's life; at least sixty-six theatrical productions were staged between 1850 and 1900. Moreover, mid-nineteenth-century writers meticulously attempted to represent other periods in German history, such as the Middle Ages, which in earlier decades had been viewed

romantically. Josef Victor Scheffel's *Ekkehard: A Tale of the Tenth Century* (1855) achieved incredible sales (the 200th printing by 1904), and Paret analyzes its popularity as emblematic of new political attitudes between 1848 and unification. The author characterizes this attitude as the "embourgeoisement of the Middle Ages" (p. 147).

Paret devotes the last section of his book to Theodor Fontane, Anton von Werner, and Adolf Menzel, who, in their depictions of contemporary history, shaped Imperial Germany's perceptions of the present as well as the past. He concludes by briefly summarizing the legacy of artistic representations of liberalism's failure in nineteenth-century Germany. For the educated public who read Kugler, Fontane, and Scheffel and saw the paintings of Menzel and Werner, the works served as sources of historical information about the national past as well as creative interpretations that influenced contemporary perceptions of the nation's present. Paret's volume is a carefully nuanced and subtle treatment of an important topic.

MARION F. DESHMUKH
George Mason University

LAMAR CECIL. *Wilhelm II: Prince and Emperor, 1859–1900*. Chapel Hill: University of North Carolina Press. 1989. Pp. xx, 463. \$39.95.

Some years ago historians who wrote about the Kaiserreich began to ignore Wilhelm II himself. Recent historians, however, have rediscovered the importance of the last Kaiser, as evidenced by a spate of publications on Wilhelm and his entourage, most notably the important studies of Isabel Hull and John C. G. Röhl. Lamar Cecil's book, the first of two projected volumes, is an important addition to this growing body of literature and will also probably turn out to be the most "traditional" of these studies. It is a straightforward narrative account of the first forty-one years of Wilhelm's life, which the author emphasizes at the outset is "a life rather than a times, a biography rather than a work of history" (p. xiii). He justifies his approach by insisting that, although Wilhelm II was at the center of the decision-making apparatus of the German empire, he was also largely detached from his own society "of which his knowledge was at best superficial and his interest only spasmodic and uninformed" (p. xiii). Disputes, debates, and discussions with other biographers of Wilhelm II and historians of Wilhelmine Germany are thus largely banned from the narrative and relegated to the endnotes, which enhances the book's readability but may detract from its utility.

Cecil's volume is the most detailed and scrupulously researched biography of Wilhelm II that has so far appeared. Cecil has made superb use of the primary records available, including a number of published documentary collections, such as the important and well-known papers of Friedrich von Holstein and Philipp zu Eulenburg. The use of the Royal Archives in

Windsor enabled Cecil to make the book's first chapters its most compelling and original. They shed a great deal of light on Wilhelm's education, the complicated and important role of his tutor, the personality and the short reign of his father, and his turbulent relationship with his mother, an unpleasant woman whom he resembled in so many ways. Nevertheless, Cecil remains reluctant in this case and others to evaluate the psychological bases of Wilhelm's behavior. For example, he is skeptical of the notion that Wilhelm's crippled arm affected his relationship with his parents, while he dismisses as "suggestive but unpersuasive" (p. 358) several arguments that have been adduced for Wilhelm's supposed homoeroticism.

Cecil's treatment of the first dozen years of Wilhelm's rule is unfailingly meticulous and always supported by massive documentation. Especially interesting is his treatment of Chancellor Hohenlohe, who emerges from the book as an effective political leader with a substantial record of achievement and an exceptional ability to respond subtly and creatively to his master's erratic personality.

Although the author's account is essentially narrative and descriptive, he does not entirely avoid the major areas of historiographical controversy concerning Wilhelm II's place in the history of the Kaiserreich before 1900. In assessing the issue of the *persönliches Regiment*, for example, Cecil tries to steer a middle course between the views of Röhl and of Hans-Ulrich Wehler, arguing that the role into which Bernhard von Bülow "entered in 1897 does not mark either the establishment of a *persönliches Regiment* or the triumph of some sort of bureaucratic or polycratic hegemony. German politics after 1888, and after 1897, was an exotic and unsettling farrago of both royal caprice and bureaucratic adjustment" (p. 261).

In the end, Wilhelm II remains an elusive figure, one who, as the author admits in the book's opening lines, still "manages to confound and annoy those who must deal with him" (p. xi). Despite his own modest disclaimers, Cecil's biography represents a substantial contribution to the literature on the Kaiserreich and whets our appetite for the second volume. Unfortunately, the publisher, presumably motivated by cost-cutting considerations, has included only a rudimentary bibliography, which forces the reader to swim through pages of endnotes in search of secondary references.

DAVID E. BARCLAY
Kalamazoo College

MICHAEL JOHN. *Politics and the Law in Late Nineteenth-Century Germany: The Origins of the Civil Code*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1989. Pp. viii, 284. \$64.00.

Law is one of those subjects that, despite its obvious significance, remains on the margins of most historical writing. Like the histories of medicine, technology and, until recently, war, the history of law has been written

by and for insiders. Michael John's fine study of the political origins of the German Civil Code vividly displays the need to move away from this deplorable state of affairs. Law is surely too important to be left to the lawyers.

John's book traces the evolution of the Civil Code (Bürgerliches Gesetzbuch) through a twenty-two-year process of drafting, public controversy, and legislative adoption. John begins with three background chapters on the theory and practice of codification before 1867; here the complex and polysemous ideas of Friedrich Carl von Savigny are especially important. He then examines the efforts of the five members of the first codification commission, who worked from 1874 until 1888 to produce a preliminary draft. After a chapter on the public debate over this draft, he follows in some detail the government's efforts to revise the code from 1888 to 1895 and then describes its final parliamentary approval in July 1896.

Two sets of questions dominated discussions inside and outside the small circle of lawyers and civil servants who were responsible for writing the code. First, how should the national code relate to the tangle of state and local laws already in force throughout the newly created Reich? Second, how should the code reconcile the traditional rights and privileges of particular groups with the principles of legal equality and individual freedom? John shows that there were deep divisions in German society about these matters. Small states feared that the code would be a kind of Prussianization, Catholics opposed compulsory civil marriage, agrarian interests wanted to have landed property dealt with separately, and various associations demanded special consideration. The code's opponents were numerous but divided; they shared nothing beyond their opposition. The code's sponsors, on the other hand, could draw support from a broad consensus in both the government and the society that some form of codification was essential. Faced with this consensus, no major party was willing to accept responsibility for defeating the enterprise. In the end, the code passed the Reichstag by a vote of 232 to 48; only the Social Democrats and a few scattered right-wing intransigents remained opposed.

As John writes in his introduction, the purpose of his book is to use the legal system "as a way of looking at the changes in the main components of Germany's political culture in the second half of the nineteenth century" (p. 14). Many of the changes he describes are familiar enough, namely, the increasing importance of parties and public opinion, the growth of interest groups, the declining cohesion of National Liberalism, and the increasing significance of the Center party in the Reichstag. But the larger picture that emerges from John's account does not easily fit with conventional portrayals of Wilhelmine Germany. The process of codification does not reveal a crisis-ridden system that was desperately trying to block the implications of modernity but rather a relatively successful system that could manage its divisions, make compromises, and

ultimately create a consensus on essential political and social issues. This system combined intense popular agitation and effective administrative control; thus, the code was essentially the work of lawyers and civil servants who listened to but were not dominated by interest groups and parliamentary parties. In sum, the political culture that produced the code represented what John calls "bureaucratic, non-participatory liberalism" (p. 248).

John is somewhat indecisive and uncertain about how his analysis fits into current controversies over the so-called German *Sonderweg*. In his final paragraph, for example, he maintains that "it is far from clear that liberalism as a set of political values was dead in Germany before 1914." There is, he goes on, "much to be said for the view that German liberalism—as a theory and as a set of political practices—failed because of the intolerable strains arising out of the First World War." He then admits, however, that the Civil Code was "perhaps already out of date by the time it was introduced" (pp. 256–57), thereby implying that the political culture from which it sprang would not have been able to survive no matter what happened. But, although John does not provide a fully satisfactory statement of the code's significance for German developments after 1896, his book does demonstrate that any adequate understanding of Wilhelmine politics and society will have to include a careful consideration of the legal system. Let us hope that others will follow John's good example and pursue further the questions he raises about law's ideological, political, and social meaning.

JAMES J. SHEEHAN
Stanford University

KEITH TRIBE. *Governing Economy: The Reformation of German Economic Discourse, 1750–1840*. New York: Cambridge University Press. 1988. Pp. x, 241. \$44.50.

Keith Tribe's book will enhance his well-deserved reputation as one of our most provocative historians of economic thought. His study painstakingly reconstructs the origins of economics as German university students were taught to understand it in the late nineteenth and early twentieth centuries. We know that German economics was once recognizably different from its British and French counterparts. *Nationalökonomie* (political economy) stressed a close correspondence between economic life and the broader life of the nation. (Max Weber's inaugural lecture as professor of economics at Freiburg in 1897 remains perhaps the most forceful expression of the distinctively German view that economics must focus on a self-evident symbiosis of state and economy.) We know, too, when and how German economics disappeared. Policies initiated by the Nazis in 1933—in this domain as in others—led to the destruction of one of the many Germanic traditions they supposedly had set out to restore. Until Tribe's book, however, we have known remarkably

little about the exact historical origins of the tradition of *Nationalökonomie*.

Why this gap in our knowledge? Tribe provides at least two answers. First, earlier scholars were apparently reluctant to work their way through a vast body of primary sources—in this instance, hundreds of textbooks, written for the most part by utterly obscure authors for the specific purpose of teaching courses in economics at German universities. Second, a flawed methodology has conceived the history of economic thought largely in terms of an evolutionary model of development. This approach typically gives priority to supposed progress in economic analysis, where "progress" means anticipating technical advances in the field over the last fifty years. The chief flaw here, Tribe argues, is that few (if any) of the now defunct texts in German economics represent contributions to the history of economic thinking conceived in this way. Hence, in his book, he "does not take the high road of progress and theoretical innovation" (p. 7) but instead follows the more mundane, yet historically concrete, course of development taken by economic pedagogy within the institutional structure of German universities. The result is unquestionably a pathbreaking study in the variety of factors that make up "the culture of economics—not simply theoretical, practical, and descriptive principles but the role and organization of teaching, recruitment to the professions, the application of economic knowledge, and the establishment of professional and academic associations" (p. 3, n. 6).

Against this book's many merits, which include an intriguing account of the German reception of Adam Smith's *Wealth of Nations* (1776), I pose one brief objection concerning Tribe's repeated contention that the texts he has studied still have much to offer contemporary economists. With the exception of a brief and familiar complaint about the narrow procedures that currently define the discipline, Tribe provides no sustainable explanation of the present-day relevance of material that he himself recognizes to be utterly different in its arguments and preoccupations from contemporary economic thought. This book, nonetheless, has much to teach students of German thought, the history of economic thought, and the cultural organization of intellectual life.

RICHARD F. TEICHGRAEBER III
Tulane University

JOAN CAMPBELL. *Joy in Work, German Work: The National Debate, 1800–1945*. Princeton: Princeton University Press. 1989. Pp. xi, 431. \$37.50.

In this history of changing German concepts of work between 1800 and 1945, Joan Campbell provides a well-documented overview of a significant intellectual debate. She starts her study in the early nineteenth century, arguing that a "modern" attitude toward work—one that recognized that work was a significant and even potentially valuable part of human exist-

ence—preceded industrialization in the German area. Classical humanists, philosophical idealists, and romanticists shaped the context of the nineteenth-century German discussion of work.

Socialists and bourgeois reformers began the debate, both groups responding initially to industrialization abroad. Karl Marx developed the idea of “joyful labor” as a goal for revolutionaries; mid-nineteenth-century bourgeois reformers such as Wilhelm Heinrich Riehl appropriated Marx’s notion as a means of averting rather than stimulating social upheaval. Riehl also added another key concept, “German work,” to the discussion in his *Die deutsche Arbeit* (1861). Of limited impact when first published, the book eventually influenced many German thinkers: by the late nineteenth century, earlier bourgeois fears of the societal impact of industrialization appeared well founded indeed, and the idea that Germans had a special mission and worked in different, superior ways from others gained in appeal. German industrialization also paved the way for the emergence of a new “science of work” before 1914. Campbell’s treatment of the German roots of the thinking of Harvard industrial psychologist Hugo Münsterberg is especially interesting.

The heart of the book involves the particularly rich German debate on work in the Weimar period, fostered for the most part by the legacy of World War I. Revolution and cultural ferment opened the way for utopian ideas and radical experiments. Attempts to “rationalize” firms in order to meet foreign competition led managers to seek new ways to motivate, subjugate, or eliminate labor, while the field of industrial relations emerged from desires to “humanize” work. Increasing application of science to industry entailed psychological testing and development of “scientific” surveys to determine workers’ attitudes. More women in the work place fundamentally altered some long-held assumptions in the intellectual discussion. The advent of the depression intensified and radicalized the debate.

In the short term, the right wing won. Campbell’s description of the development of the idea of work in the National Socialist period is generally well done: the idea clearly was significant, being bound inextricably with Nazi nationalist and racial notions. The key role of the German Work Front (DAF) in the early Nazi period makes this clear; the sign at the entrance to Auschwitz, “Work makes free,” provides chilling confirmation of the importance Nazis attached to the concept.

Unfortunately, Campbell stops her treatment in 1945. Although her reasons for not going into the postwar period are convincing, she provides tantalizing evidence that this is a subject that should be pursued further. In particular, the possible implications of her study for Volker Berghahn’s thesis in *Americanisation of West German Industry* (1986) are massive. Although Campbell suggests that the National Socialists emphasized the peculiarities of “German work,” she argues that the regime of wartime necessity implemented

more of the American system of mass production in pre-1945 Germany than Berghahn might allow.

It should be obvious that this book covers much familiar territory. Yet it does so from a previously unexplored perspective, the social history of the idea of work. It thus potentially could alter previous preconceptions and perceptions. Certainly, historians in a wide variety of fields will find this book worth consulting: besides appealing to political and intellectual historians, Campbell touches on issues in business and economic history, women’s history, and the history of science, technology, and psychology.

Yet Campbell’s strength is at times a weakness. Her forays into the political implications of the intellectual debate are tentative and often inconclusive, and she discusses only briefly its impact on industrial or technological development in Germany. Still, Campbell’s fine book will prove an invaluable resource for more specialized discussions relating to the problem of work in German society.

RAYMOND G. STOKES
Rensselaer Polytechnic Institute

RUDOLF BERTHOLD, editor. *Geschichte der Produktivkräfte in Deutschland von 1800 bis 1945*. Volume 3, *Produktivkräfte in Deutschland 1917/18 bis 1945*. Berlin, G.D.R.: Akademie. 1988. Pp. 566. 68 M.

This is the third and last volume of a series dealing with the productive forces in Germany between 1800 and 1945. Its contents are organized exactly as those of volume 2. This book covers the period from 1917/18 to 1945, that is, from the middle of World War I to the end of World War II. In both volume 2 and volume 3, chapter headings are identical. General propositions are discussed in the first chapter; sections on industry, agriculture, transportation and communication, location, the natural sciences and mathematics, education and vocational schools, and, lastly, labor follow. The book is a compilation of chapters, each produced by one or more persons, many of them members of the Economic History Institute of the Academy of Sciences of the German Democratic Republic.

The volume is on the whole a straightforward collection of economic matters. With numerous tables and many pictures, it gives a good view of the period. It is somewhat marred by the use of code words, which are assumed to be known by the reader and are therefore never fully explained. For example, the Nazi period is referred to as the time of fascism. The terms “Nazi” or “National Socialist” are hardly to be found in the entire book. That choice seems not to have anything to do with Marxism, although use of other code words does. Here we meet quite often the term “monopoly capitalism,” which also requires definition for those untrained in the field of Marxist, East German historiography. For example, one would like to know what the authors have in mind when they talk about state monopoly conditions of the Weimar Republic. Although these

terms are not completely strange to those reading the East German literature, one would still like to see them backed up with more evidence.

In part this book is encyclopedic. It shows the progress made by the various industries and agriculture. In indicating the state-of-the-art conditions of industries and agriculture, it does not, however, show how widely technological artifacts were adopted.

There is no question that many parts of this book are strongly influenced by Marxist doctrine. Nevertheless, to read about the development of imperialism in which a dying capitalism has transformed itself into a capitalism that is already partially dead is a bit of an anachronism, especially given the changes that are taking place in today's Communist world. But the book is still extremely valuable in that it brings together in one volume so many facts and figures. Some chapters fit in a rather odd way. It is a bit peculiar that, in what seems to be a treatise on economic and technological history, one finds a chapter on the natural sciences and mathematics. To be sure, from time to time in that chapter, an effort is made to show the connection between industry and science. Not surprisingly, industrialists preferred applied science to pure science. One gets a similar feeling of a combination of relevant and irrelevant material being brought together in the chapter on education. It almost seems as if the contributors to this volume were assigned a particular topic and told to write a given number of words. In this way some topics may have been extended beyond what seems reasonable. Overall, I wish to recommend this book as well as the preceding volume for the great deal of substance that is found in them.

HERMAN FREUDENBERGER
Tulane University

IAN KERSHAW. *The 'Hitler Myth': Image and Reality in the Third Reich*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xii, 297. \$55.00.

Ian Kershaw has written an important book. A companion volume to the author's *Popular Opinion and Political Dissent in the Third Reich: Bavaria 1933-1945* (1983), it was originally conceived as a component of the Munich Institute of Contemporary History's multi-volume project, *Widerstand und Verfolgung in Bayern 1933-1945*. Extensive research in German national, state, and provincial archives gives the work a solid foundation. Much of it is based on an analysis of the weekly reports on morale found in the extensive Sicherheitsdienst (SD) files, the Sopade reports compiled by the exiled Social Democratic Party of Germany (SPD), and the judicial reports of Munich's notorious Sondergericht. Kershaw views the Hitler myth as the cohesive element offering unity to a nation torn apart by acrimonious political, economic, social, and cultural divisions. This theme is disarmingly simple but is used by the author to weave variations of conceptual subtlety. The work is organized into three major sections.

It begins with the making of the Hitler myth, 1920 to 1940, continues with its breakdown during World War II, and concludes with a chapter entitled "The Hitler Myth and the Path to Genocide."

Throughout the book, Kershaw demonstrates the stark contrast between the image of the Führer and reality. From the first, he was viewed as the personification of the nation, diametrically opposed to the hordes of new-rich plunderers of the German people clustered in over thirty political parties, none of whom had the foggiest notion of the heroic spirit that motivated the fallen of Langemarck and Verdun. Once Hitler took power, this myth shielded him from criticism. Those with grievances did not condemn Hitler but, instead, his subordinates in the party. Those who did not agree were intimidated into submission, which contributed to the growing consensus in favor of Hitler. He seemed to embody the spirit of the nation, sent by Providence to return his people to economic prosperity. The public generally approved of Hitler's forceful handling of the Röhm crisis, thereby underwriting his ruthless pursuit of a redefined national morality.

Kershaw submits that Hitler was able to pose as the protector of the little man and woman against the "radicals" in the party, most notably in the battles over church policy. The arrest in 1934 of Hans Meiser, the popular Protestant bishop of Bavaria, led to widespread protest, and Meiser was returned to office in a matter of weeks. Further, during World War II, the party had the audacity to order the removal of crucifixes from the schools even as masses were being said in Germany for the souls of the fallen in the killing fields of Russia. The firestorm of protest that followed once more enabled Hitler to appear as the white knight. Hitler's extraordinary successes in foreign policy from 1935 through 1938 added immensely to the Hitler myth that peaked after the annexation of Austria. Hitler could celebrate his fiftieth birthday in April 1939 knowing that "the great majority of Germans could find some point of identification with his achievements" (pp. 140-41).

The Hitler myth was threatened only when the public was called on to make sacrifices during World War II. Disappointment with the length of the war, irritation with the scarcity of coal and meat, and fear that the campaign in Russia could end disastrously contributed to growing discontent. Jubilation over the Blitz victories of 1939 and 1940 gradually gave way to criticism of the Führer himself, and those guilty were charged with high treason. The Hitler myth was slow to die, however, because Hitler "had become the projection of national aspirations to greatness" (p. 171). Joseph Goebbels was successful in his assault on "Jewish-Bolshevism," and Hitler remained a symbol of hope and determination for millions of people.

Kershaw is to be commended for this work. The author, however, should have treated the problem of the "Final Solution" with more sensitivity. It is not enough to claim that "anti-Semitism was for the most part of no more than secondary importance as a factor

shaping popular opinion in the Third Reich" (p. 230). Here as elsewhere anecdotal evidence collected by the SD and the SPD in exile is accepted without the skepticism that such sources demand. Nevertheless, Kershaw's study is a significant addition to the literature available on Adolf Hitler.

JAY W. BAIRD
Miami University

RAYMOND G. STOKES. *Divide and Prosper: The Heirs of I. G. Farben under Allied Authority, 1945–1951*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xiv, 290. \$40.00.

Late in 1948, the research director of the American Hercules Powder Company stated that "Germany can never regain the dominant position it once held as a chemical producer in world markets." He could not have been more wrong. By the time he made his remarks, Germany's currency had been reformed, and help had begun to arrive in the form of the Marshall Plan. What had been a slow recovery became a rapid one, and a few years later the German chemical industry began a resurgence that has not stopped to this day.

Raymond G. Stokes's monograph lucidly traces this rise from the ashes of West Germany's chemical industry. In 1939 I. G. Farben was the largest chemical firm in the world and the largest industrial corporation in Germany. In 1945 the British gained possession of Bayer at Leverkusen, the Americans of Farbwerke Hoechst at Frankfurt—together with the corporate head office—and the French of the Badische Anilin- und Soda-Fabrik (BASF) at Ludwigshafen. These three companies were both the major precursors and the successors of I. G. Farben, which hints at the continuity that prevailed in spite of Allied intentions.

How to control the postwar German economy had been a topic in Allied government circles even before the end of the war. The conviction that German industry had been more than a willing accomplice in Hitler's nefarious designs spawned the Morgenthau Plan, which wanted to turn Germany into a pastoral weakling. The Treasury Department's and the Anti-Trust Division's strong views concerning I. G. Farben were reflected in the order that directed American authorities to seize the company's property and assets, to make them available for dismantling and reparations, to destroy war-related production facilities, to suspend the rights of shareholders, to remove the management, and to disperse ownership and control. French and British policies were remarkably similar, wishing also to exploit German industrial expertise and curb future German competition. The major difference among the three zones was that, whereas Bayer and Farbwerke Hoechst had escaped the ravages of war almost totally, bombing had reduced BASF's production capacity to 40 percent.

Whatever the political and economic intentions of the Allies toward Germany's chemical industry might

have been, they were soon altered by the conditions of the defeated country. First, the problem of how to pay for the occupation could only be solved by rebuilding German industry and promoting exports. Second, political tensions between East and West forced the three Western Allies into greater cooperation. The economic merger of the British and American zones in January 1947 was followed in June 1948 by the German currency reform. At about the same time the Marshall Plan began to exercise its beneficial effects.

Also in 1948 steps were taken to disperse I. G. Farben; by 1951 its legal breakup was complete. Today the three independent successor companies of Bayer, Farbwerke Hoechst, and BASF are the second, third, and fourth largest chemical companies in the world (after Du Pont). Each of the three is now larger than I. G. Farben ever was.

Divide and Prosper is therefore an eminently fitting title for a story that describes in fascinating detail the fall and rise of the German chemical industry, a story for which the author has thoroughly consulted all relevant documents and literature. It is a valuable contribution to economic history that no one interested in German history ought to miss.

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WERNER BÜHRER. *Ruhrstahl und Europa: Die Wirtschaftsvereinigung Eisen- und Stahlindustrie und die Anfänge der europäischen Integration 1945–1952*. (Schriftenreihe der Vierteljahrshefte für Zeitgeschichte, number 53.) Munich: R. Oldenbourg. 1986. Pp. 236. DM 28.

For many people the main interest in Werner Bührer's book will be the additional light it throws on the attitude of the German iron and steel industry toward the negotiations for the European Coal and Steel Community. But this issue is the concern only of the last part of the volume. The work as a whole is much wider. It attempts to see the industry's response to the Schuman proposals in the whole context of postwar restrictions on output, dismantling of productive capacity, removal on political grounds of some of the leading managers, and both Allied and German legislation to reduce the size of companies. In addition to material gleaned from Bührer's diligent search through the trade press and local archives, the book rests on three new sources of evidence: the personal files of two important postwar figures in the industry, Wilhelm Salewski and Günter Henle, and the records of its trade association, the Wirtschaftsvereinigung Eisen- und Stahlindustrie. The great size and complexity of the industry means, as the author readily accepts, that the official expressions of opinion of the trade association and even of its leading figures are not necessarily an accurate historical guide to opinion throughout the sector. Even these quasi-official expressions of opinion were, in fact, varied. This was especially the case once the industry was allegedly, though

not really, offered equality of treatment by the Schuman proposals. The only issue on which it was united during the negotiations for the Treaty of Paris was to reduce the powers of intervention and control of government, whether national or international. For most of its representatives, as for their foreign counterparts at the negotiating table, the ideal arrangement would have been a return to the loosely structured international cartel of the interwar period. This solution was clearly unacceptable as the basis of what was in effect both a peace treaty and an intended new political framework for a future Europe. The solution was also unacceptable to postwar government with its sweeping domestic political ambitions. For most of the period with which the book deals, therefore, we see the industry engaged in an exercise to limit what it saw as serious damage. During the negotiations for the Treaty of Paris, the industry's representatives were sometimes able to do this in agreement with French negotiators, excluding the issue of export prices and a wide range of steel-processing activities from the purview of the High Authority, for example. But sometimes the industry's representatives failed, as when the French continued to insist that the High Authority should have powers to "harmonize" input costs and sale prices. The figures who came to the top of the industry were those who realistically accepted the new situation. Günter Henle acted as advocate within the trade association for Konrad Adenauer's policies. Heinrich Dinkelbach became one of its chief executives. Hermann Reusch, by contrast, ended such political influence as he still exercised by denouncing these policies and leaving his role in the negotiations. As in France, the postwar exigencies of international and of national politics ended by imposing significant changes on the industry's leadership. Given the close association between the central government's ambitions and the steel industry in so many European countries after 1945, this is not surprising. Bühner's work should be considered a valuable beginning to the study of a topic that is likely to remain an important zone of historical interest.

ALAN S. MILWARD

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G. C. J. J. VAN DEN BERGH. *The Life and Work of Gerard Noodt (1647–1725): Dutch Legal Scholarship between Humanism and Enlightenment*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xiv, 391. \$88.00.

In the best tradition of books dealing with "life and works," this book begins with a long chapter on the life and career of Gerard Noodt, and G. C. J. J. van den Bergh succeeds in placing Noodt in his intellectual and social context.

An even longer chapter deals with each of Noodt's works in chronological order. It begins with an attempt to place the Dutch Elegant School in the historical development of an elegant tradition in Roman legal scholarship. That tradition was closely connected to the

rise of a humanist interest in *antiquitates* and *elegantiae*, which were used by jurists as tools in the interpretation of texts. But that concern with language and meaning also led to the possibility that Roman law itself had changed through history. The text now not only had to be submitted to historical criticism but also put under the scrutiny of a rational criticism. As Jacques Cujas had said over a century before Noodt, *ratio juris* often carried more weight than any manuscript, which could easily be corrupted. The result of his historicization and rational criticism was "that the text of the Corpus Juris no longer had final authority in legal matters."

Van den Bergh shows how Noodt's "method" relied heavily on interpretation and emendation of texts and how, in fact, he was often considered rather audacious in his readings. This concern for philological elegance coupled with an acute awareness of the historical development of Roman law and a strong rationalist tendency make Noodt one of the leading figures in the Dutch Elegant School, which dominated the legal field at the beginning of the eighteenth century.

Noodt was and is best known outside the field of Roman law for his two rectorial orations: the first in 1699 on sovereignty and the second in 1706 on freedom of religion. Both were reprinted many times and widely translated into the 1780s. The essay on sovereignty undertook to determine whether the ruler was subject to the law or above it and concluded that all rule is limited and that the people have inalienable rights. The last translation of this work into Dutch was in 1784, when Patriot ideology was being elaborated, and Noodt thus became, in the politics of that time, another defender of popular sovereignty.

In the oration on freedom of religion, Noodt went far beyond the concept of toleration. This address has an important place in the history of the Enlightenment and was often linked with John Locke's work on toleration. In this radical work Noodt argued that freedom of religion should be absolute, free from all interference by the government, and that there should be no exceptions to this right. As a jurist, however, he understood that Roman law could not serve as an authority in this case and so based his legal arguments entirely on natural law.

Van den Bergh has written a learned and elegant book worthy of its subject, and he has opened up a hitherto obscure aspect of Roman law, including many interesting observations on Dutch university life. By not giving sufficient weight to the continuity of the controversies in Roman law, perhaps he emphasizes too much Noodt's originality. Bartollus, after all, three and one-half centuries earlier, first made many of the same arguments about law and revolutionary political claims against the power of the sovereign. But that fact should not detract from Noodt's well-deserved reputation as a "jurist in his own right with full consciousness of his social responsibilities."

I. LEONARD LEEB

Polytechnic University

HANS NORMAN and HARALD RUNBLOM. *Transatlantic Connections: Nordic Migration to the New World after 1800*. Oslo: Norwegian University Press; distributed by Oxford University Press, New York. 1988. Pp. 335. \$29.95.

In 1960, at the Eleventh International Congress of Historians in Stockholm, Frank Thistlethwaite appealed to European colleagues not to leave the history of the trans-Atlantic migration entirely to New World scholars. The response since then has been impressive, not least in the Nordic countries. With rare exceptions, such as Ingrid Semmingsen in Norway and Helge Nelson in Sweden, academic scholars in the homelands had shown scant interest in their emigrated compatriots. Yet the Nordic lands were ideally situated for such research. Vital statistics had long been meticulously kept in these countries, and the 2.5 million Nordic emigrants, from the beginning of their mass migration in the 1830s, were overwhelmingly literate and produced a sizable documentation. Moreover, because of close similarities in language (except for Finnish), culture, and historic background, Denmark, Finland, Iceland, Norway, and Sweden offer a uniquely valuable field of comparison.

During the 1960s, migration research in Scandinavia blossomed, most notably at Uppsala University in Sweden, where, between 1962 and 1976 especially, many younger scholars in the field, including Hans Norman and Harald Runblom, wrote and published their dissertations. The Nordic research of the 1960s and 1970s generally stressed emigration, social scientific methodology, quantification, and teamwork, which included inter-Nordic cooperation resulting in maps of emigration intensity for the entire region and inter-Nordic symposia with published proceedings.

In 1976, *From Sweden to America*, edited by Norman and Runblom, summarized Swedish emigration research during its most intensive phase. Surprisingly, meanwhile, no summary of such research throughout the Nordic region appeared in any language. This deficiency Norman and Runblom correct, building on a voluminous monographic literature. They even-handedly treat migration from five different lands; if the Swedes appear at times to predominate, this simply reflects their greater absolute number among the emigrants, as well as their production of the largest quantity of published research.

The contributions of the two authors offer certain contrasts. Norman, in dealing with emigration from the homelands, relies on quantification and theoretical models, derived from research mostly completed before 1980. Runblom, writing on Nordic immigrants in the receiving countries—principally but not only in North America—lacks a comparable statistical base and thus follows a more qualitative, humanistic approach, in common with most American scholars of ethnic life. This focus became increasingly characteristic of migration studies in general during the 1980s, and the

insights offered in this section seem rather fresher to the initiated reader.

For those who have not closely followed this vital field of regional migration research, Norman and Runblom provide a useful and illuminating survey, placing their findings into the context of international migration research. Few and only minor errors are noted, but documentation raises some questions. The extensive bibliography naturally includes Nordic works in their original language but fails to indicate that many have also appeared in English. Notes in the second half of the book often cite publications but not page numbers. Proofreading could have been better. But these details scarcely detract from the value of this welcome—and surely overdue—survey of an admirable body of scholarship.

H. ARNOLD BARTON
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TORKEL JANSSON. *Agrarsamhällets förändring och landskommunal organisation: En konturteckning av 1800-talets Norden* [Agrarian Change and Rural Local Government: An Outline of Nineteenth-Century Balto-Scandinavia]. Summary in English. (Studia Historica Upsaliensia, number 146.) Stockholm: Almqvist and Wiksell International. 1987. Pp. 229. 162 KR.

This is the fourth volume in an inter-Nordic series titled "Social Change and the Origin of the Modern Association Movement." Indeed, it is Torkel Jansson's second book in the series; his first, *Adertonhundratalets associationer* (1985), was reviewed by Byron Nordstrom in the *American Historical Review* 92 [1987]: 690. His second work treats the creation of self-governing rural municipalities in the nineteenth century within a socio-economic context that Jansson describes as the transition from feudalism to capitalism.

Jansson's use of the term *Norden* ("the Nordic countries") is idiosyncratic and disconcerting. In normal Scandinavian usage, the term encompasses—and is limited to—the five sovereign states of Denmark, Finland, Iceland, Norway, and Sweden, as well as the semiautonomous Åland and Færoe islands, all of which cooperate within the framework of the Nordic Council. Yet, in *Adertonhundratalets associationer*, Jansson took the liberty of including the Russian empire's Baltic provinces in the sphere of *Norden*, and in this book he expands *Norden* once again to include Schleswig-Holstein. In effect, Jansson himself acknowledges the inappropriateness of his use of *Norden* in the Swedish title when he replaces it with the neologism *Balto-Shandnavien* in the book's German summary ("Balto-Scandinavia" in the English abstract).

The problem that fascinates Jansson is how local self-government developed in rural society in the gray area between the state and the private sphere. He explores the dilemmas faced by central government officials and local leaders alike in shifting gears between

early modern concepts of state-directed local government and an emerging liberal ideology that favored voluntary self-help organizations and private enterprise over state action in dealing with many societal affairs. As new legal institutions were (sometimes haltingly) being developed to accommodate such innovations as limited risk business corporations, it was simply not clear to many officials and citizens whether local self-governments were to be viewed as voluntary associations or institutions to be regulated by public law.

Two of Jansson's examples of the indeterminate status of self-governing rural municipalities between the spheres of public and private law, one from Sweden and one from Finland, capture in a nutshell the nature of the problem. As a hedge against poor harvests, parish granaries were established in Sweden at royal initiative in the eighteenth century, but a government circular of 1834 concerning these institutions declared them "to be 'the property of private individuals,' but as they had been established 'for common benefit' and affected 'what is perhaps an important aspect of the kingdom's economic affairs,' they should nonetheless not 'escape public scrutiny'" (p. 102). Equally illustrative is the fact that letters from self-governing rural municipalities were registered by provincial governors and governors-general in the grand duchy of Finland under the rubric "private documents" (p. 149).

In his separate surveys of nineteenth-century developments toward local self-government in rural society in the Russian empire's Baltic provinces, Denmark, Schleswig-Holstein, Iceland and Færoe Islands, Greenland, Norway, Sweden, and Finland, Jansson raises important questions about the changing nature of law, society, and the state. Unfortunately, the book is written in a very heavy-handed style that could have benefited tremendously from copyediting, something all but unknown in Swedish academic publishing. There is a thirty-five-page German summary.

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PETER ENGLUND. *Det hotade huset: Adliga föreställningar om samhället under stormaktstiden* [A House in Peril: The Conception of Society in the Nobility during the Age of Greatness]. Summary in English. Stockholm: Atlantis. 1989. Pp. 344.

In 1988 Peter Englund, a doctoral candidate at Uppsala University, created a sensation in Sweden with a widely hailed book on the Battle of Poltava. The following year he defended his dissertation—here reviewed—before a large public. Englund traces changes in the social "ideology" of the Swedish nobility during the seventeenth century, drawing on an impressive official, public, and literary documentation. His terms of reference are broadly Marxian, while his approach bears the impress of the new social history.

The seventeenth century witnessed Sweden's mete-

oric rise to great power status. The rapid evolution of the military state with its administrative and economic needs led from the 1640s on to new ennoblements on a scale unparalleled in Europe, principally from the burgher estate.

In 1600 the Swedish nobility still comprised a small, exclusive landowning elite, whose value system stressed a strict functional separation of estates, identified virtue with lineage, opposed change as socially dangerous, and was patriarchal and antimercantile. Functional distinctiveness of the estates, Englund argues, was a relatively recent ideal, at least in Sweden, arising from a late sixteenth-century aristocratic reaction against a less restricted society, reinforced by the initial leadership needs of the military state.

Growing state demands essentially transformed the nobility. By 1680, when the old high aristocracy was significantly weakened by royal absolutism and reclamation of alienated crown lands, some 80 percent of the estate consisted of new nobles, the majority of whom were not landowners and thus dependent on state service. The result, Englund holds, was a "historic compromise" between "feudal" and "bourgeois" values (p. 243). The new composite nobility still upheld a hierarchy of estates and a patriarchal relationship with the peasantry. But it openly espoused mercantile activity and investment in production rather than ostentation. Above all, it moved away from the functional separation of the estates toward an individualistic, meritocratic view of nobility as the rightful reward for personal distinction in any significant field, economic or cultural as well as military or administrative. The upper bourgeoisie drew closer to the new nobility but further apart from the tradesmen and artisans of its own estate. Increasingly, function determined status rather than the reverse.

Englund intentionally leaves aside political debate and foreign influences. These are significant omissions, which leave relevant questions unanswered. Although he points to parallels elsewhere in Europe, England does not address the attitudinal impact of Sweden's large-scale ennoblement of foreigners. Nor does he closely relate the changing composition of the nobility to specific political developments. He thus offers no real explanation of how so exclusive, traditionalistic, and politically powerful a group as the old landed nobility could accept and rationalize the opening of its ranks to a flood of outsiders threatening to its traditional values. We are told what happened, in ideological terms, but not always why.

Such reservations notwithstanding, this is an important contribution to early modern studies by a promising young historian. It is on the leading edge of social history yet shows a more humanistic bent than most Swedish historical scholarship in recent decades.

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ARTHUR FIELD. *The Origins of the Platonic Academy of Florence*. Princeton: Princeton University Press. 1988. Pp. xvi, 302. \$47.50.

This book offers a revisionist interpretation of the origins of the Platonic Academy in Florence. Earlier historians have noted the turn among Florentines from an emphasis on ethics and "civic" morality to broader and more systematic Platonic philosophical studies. Such scholars, especially Eugenio Garin, have argued that this changeover occurred during the rule of Lorenzo de' Medici (1469–92), as the Medici encouraged Platonic studies in an effort to promote political submissiveness among Florentine intellectuals. Arthur Field locates the origins of the Platonic Academy earlier, in the decade between 1454 and 1463, and finds the motivations and development to be somewhat different.

Field argues that the reasons for the turn to Platonism are to be found in the aspirations of Florentine intellectuals themselves rather than in external influences. To prove his case, he takes the reader step by step through the origins of Platonic studies. Several Florentine intellectuals sought a new philosophy that Florentine humanism, led by Poggio Bracciolini, could not provide. So they looked elsewhere. One of the best parts of the book is Field's examination of the works of lesser-known Florentines, men such as Lorenzo Pisano, Niccolò Tignosi, Donato Acciaiuoli, and Antonio and Pellegrino degli Agli. These intellectuals searched for harmony; their writings exhibited a longing for love, unity, and friendship. John Argyropoulos's philosophical lectures at the Florentine university, begun in January or February 1457, initially promised to slake the thirst for philosophy. But Field demonstrates through examination of the fifteen hundred manuscript pages of Argyropoulos's lectures that, contrary to previous views, he taught Aristotelianism rather than Platonism. Argyropoulos ultimately left his Florentine listeners unsatisfied.

Marsilio Ficino then emerged as the intellectual leader. In 1456 he began to study Greek in order to approach the Platonic sources directly. He also left his narrow Scholastic circle in favor of broader scholarly contacts with Florentine patricians. Ficino emphasized philosophy's effects on the individual; he made the connecting links between the active and the contemplative lives. Cristoforo Landino in his lectures on Dante, Virgil, and other authors, beginning in the autumn of 1456 and continuing for forty years, followed. Using poetic theory and allegory, he presented Platonic ideas and Ficinian insights in humanistic guise to an eager Florentine audience.

Field then makes an excursion into Florentine politics to demonstrate that the Medici could not have imposed or promoted Platonism in the period between 1455 and 1458, because the Medici political party was in disarray at that time. It was much stronger after 1466, by which date Platonism was firmly established in Florentine learned circles. Now Platonic concepts of

harmony and unity provided an ideology for the Medici party. This was the culmination of the nuanced and intricate historical development of the Platonic Academy.

The book is based on the most comprehensive and extensive search through the manuscript sources for this period yet undertaken. In many cases Field has discovered and used important new manuscripts, such as Landino's earliest lectures. The book is well written and occasionally witty; misprints and colloquial expressions are rare. This is a learned and convincing revisionist account of how a set of philosophical ideas with vast future influence developed in a civic context.

PAUL F. GRENDLER

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WILLIAM MCCUAIG. *Carlo Sigonio: The Changing World of the Late Renaissance*. Princeton: Princeton University Press. 1989. Pp. xiii, 380. \$45.00.

The life, writings, controversies, and reputation of Carlo Sigonio (ca. 1523–84), one of the most important Italian philologists and historians of the later Renaissance, are the subject of this extensively researched study. William McCuaig, who demonstrates a command of classical scholarship, attempts to situate Sigonio in the cultural, political, and religious world of northern Italy viewed according to the by-now outdated theses of Francesco De Sanctis and Benedetto Croce. Thus, the overarching factor that McCuaig uses to explain why the Renaissance declined or why people acted in defensive or devious ways is the all-purpose villain, a "Counter-Reformation mentality."

Sigonio's academic career is carefully traced, but his private life and personality remain a mystery despite occasional references to his student boarders, his association with the *Spirituali* and other church reformers, and his solitary ways, independence, ambition, and even arrogance.

His publications established his reputation as one of the leading classical scholars of his age and involved him in a series of controversies that allowed him to display his erudition and rhetorical skills. His early works treated the ancient Roman republic and focused on questions of citizenship, political organization, law, and administrative procedures. With Francesco Robortello, he quarreled over Roman forenames and chronology. With the French scholar Nicholas de Grouchy, he argued over the meaning of the terms *magistratus* and *imperium* and over voting procedures within the Roman assemblies. On the latter subject he also debated the Venetian patrician Bernardino Loredan. Sigonio's studies of ancient Greek and Hebrew chronology and government did not produce notable controversies. When in later life he applied the critical historical method to the study of the Christian era, Sigonio had to defend his debunking of cherished myths against the attacks of Venetian and Bolognese supporters of their local civic legends and against the

criticisms of some Roman censors, primarily Guglielmo Sirleto and his Dominican cohorts. But the controversy that ended his career under a dark cloud was his debate with Antonio Riccoboni over the authenticity of the *Consolatio*, first published in 1583. Riccoboni accused Sigonio of being the author or at least a collaborator in the forgery of this "discovered" lost work of Cicero. McCuaig supports these charges by publishing an appendix of twenty-four letters related to this controversy.

The generally favorable reception given to Sigonio's publications is explained not only by their skillful layout of useful information and innovative analyses of social and political organization and developments but also by his choice of such themes as republican liberty, citizen participation, and social evolution, which found nostalgic resonances in the changed world of later sixteenth-century Italy and beyond.

McCuaig's portrait of Sigonio as a supporter of republican values rings true, but his claim that he was a victim of the Counter Reformation must be carefully qualified. That Sigonio encountered difficulties with nervous Roman censors is not surprising, but he often won the intellectual debate or negotiated a textual compromise. His critics represented but one, and that not the dominant, strain of thought in the Roman bureaucracy; his works were never put on the Index. Powerful clerics such as Cardinal Gabriele Paleotti and Bishop Agostino Valier intervened on his behalf, and Pope Gregory XIII entrusted him with the task of responding to the Magdeburg Centuriators. Church officials did, indeed, seek to suppress heretical and suspect works, but they did not attempt to seal off Italy from all intellectual contact with the north. Sigonio was a pious man who sought to serve the church and in particular the papacy by his scholarship. He even functioned as a reader for the Venetian censors. The late Renaissance witnessed not the suppression of classical ideas, values, and forms but their entrenchment and evolution within the educational, literary, artistic, and cultural worlds of the sixteenth and subsequent centuries. McCuaig's picture of Sigonio's life and writings is remarkably detailed and clear, even if its background is occasionally distorted.

NELSON H. MINNICH
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MIRIAM TURRINI. *Penitenza e devozione: L'episcopato del card; Marcello Crescenzi a Ferrara (1746-1768)*. (Testi e ricerche di Scienze religiose, number 24.) Brescia: Paideia. 1989. Pp. 388.

If Marcello Crescenzi, archbishop of Ferrara (1746-68), was representative of the Catholic episcopate in the mid-eighteenth century, it is not difficult to understand why the shock occasioned by the French revolution and its representatives in Italy was necessary to push the church toward coming to terms with the major theological and intellectual currents of the Age of Enlight-

enment. Trained in Rome, mainly by Jesuits, Crescenzi appears to have been totally immune to both Jansenism and the Enlightenment except to conceive of both as the worst of heresies. As nuncio to France (1739-43), he exhausted his energies in hunting down Jansenists either real or imagined. Given his strictly ultramontane mentality, he was unaware of or unable to comprehend the parliamentary-Gallican resistance to his policies or the efforts of Cardinal Fleury to avoid a direct confrontation between the hostile religious factions in France.

A chapter on Crescenzi's library in Ferrara, of which Miriam Turrini gives a complete listing, speaks eloquently of the prelate's thinking. He was obviously not even near qualifying as a scholar; the majority of works in his library show an overwhelming "presenza gesuitica" (p. 65). Of almost equal importance were the spiritual writings of Leonardo of Porto Maurizio, Crescenzi's personal friend, who conducted several retreats in Ferrara during Crescenzi's episcopate.

The second section of Turrini's book deals with the religious and social conditions in Ferrara in mid-century. A chapter on the instruments of episcopal government follows, and then more than half of the book takes up the archbishop's pastoral work. This is the field in which the archbishop displayed his true virtues (after all, one does expect a bishop to act primarily as a shepherd), first by continued residence and then by scrupulously carrying out his duties within a rigid Tridentine framework. Not a hint of critical interest in the movements that make the century so vibrant seems to have touched the archbishop or any of his flock. As Turrini writes in the conclusion: "His limited capacity for political analysis and the [equally] limited opinions shown during the French nunciature but also the exclusively moralistic reading of the vicissitudes of his flock lie at the source of an absolutely closed mind in confrontation with the new sensibility" (p. 257). Despite his pedestrian mentality, Crescenzi ironically enough strove for the same high penitential standards of discipline espoused by the Jansenists whom he regarded as near satanic. Augustinian rigorism was not exclusively the property of Cornelius Jansen's followers.

In light of the obvious scholarly standards and pellucid style of the author, one is left with the feeling and regret that she was trapped with the analysis of such an ordinary prelate as Crescenzi. The book is supplied with 104 pages of elaborate graphs duplicating more or less what has already been adequately described in the main text. A printed listing of Crescenzi's library with full bibliographical detail is included as well as an index of proper names and one of places. The index of proper names is of special value because of the relative obscurity of members of Crescenzi's curia and the pastors who staffed the churches.

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GIUSEPPE ISOLERI. *L'istituzione di una camera di commercio a Genova nel dibattito politico dal 1789 al 1797*. (I tempi della storia, number 3.) Genoa: Culturali Internazionali Genova. 1987. Pp. 253. L. 25,000.

This book is an institutional and political history of the efforts of Genoese reformers to establish a chamber of commerce in the final decade of the Genoese Republic (1789–97). The study is based on primary sources, namely, verbatim transcripts of meetings of the major legislative bodies of the period, among them, Collegi, Consiglio Minore, Giunta dei Mezzi, and Giunta dei Confini. The establishment of a chamber of commerce was of major importance for the Genoese Republic because commerce, banking, and foreign investment had been a major source of economic activity since the Middle Ages. Genoa had developed an autonomous mercantile jurisdiction in the fourteenth century, the *Rota Civile*, which was a body of professional jurists and merchants with technical competence in maritime affairs, trade, banking, and insurance.

At the end of the eighteenth century, it was thought by Genoese reformers that a chamber of commerce would contribute to the commercial revival of the republic, facilitate trade, provide a more efficient means to settle legal disputes, eliminate legal intrigue, and reduce transaction costs in trade. Marseilles in the sixteenth century and Glasgow, Belfast, and Manchester in the eighteenth century had already provided historical precedents. But the failure of the aristocratic republic to establish a chamber of commerce at the end of the eighteenth century—in a period of exceptional economic and financial prosperity for the Genoese merchant class—was symptomatic of the institutional crisis that destroyed the republic in 1799.

Genoa more than most European states during the *ancien régime* suffered a sclerotic social and political system under a patrician ruling class renowned throughout Italy for its blind, immobile, and apathetic policies. Without a Gaetano Filangieri, Antonio Genovesi or Pietro Verri, Genoa was much less deeply affected by Enlightenment reform than Milan, Naples, and Florence. The bourgeoisie and merchant classes were rigorously excluded from the government in which nobles had a monopoly on political power and privilege. Yet Genoa was not without its reformers, and Giuseppe Isolero's study documents, through political debates in the Consiglio Minore, the efforts of a small group of Genoese reformers—Gerolamo Serra, Costantino Balbi, Nicolo de Mari, and Luigi Corvetto—to establish a chamber of commerce.

The chamber was first proposed by Serra in 1790 and 1791–92 as a means of increasing government revenues by taxing the merchants. The republic was threatened in this period by growing budgetary deficits and, because of the threat of invasion from Piedmont, was forced to increase defense spending. The constitution of a chamber of commerce was tabled again by Serra in 1793, when the republic was faced with a fiscal crisis, and in 1796 during the English blockade. In

1797, after the treaty that allied Genoa to France and forced the republic to make a loan of 4 million lire to Napoleon, Serra again proposed a plan for a chamber of commerce as well as a plan for a discount bank. Under this plan the merchants would have contributed three hundred thousand lire annually to the republic's income, while the discount bank, which had been proposed some years earlier by Corvetto, would have increased the supply of credit for trade and commerce. The government rejected both proposals. The plan for a chamber was declared unconstitutional because it included non-nobles (merchants) in its administration. Finally, after the collapse of the aristocratic republic in June 1798, commercial tribunals were established in the leading cities of Liguria to stimulate commerce, and a chamber of commerce was finally established in Genoa under Napoleon in 1805.

Isolero's study suffers from excessive use of quotations—verbatim transcriptions of debates in the legislative councils—and tends therefore to be excessively pedantic. Its main interest lies in the political ideas of a small but ineffective group of Genoese reformers who were overtaken by the political events of the French revolution as it spread throughout Liguria. The study deals only tangentially with nonpolitical aspects of the decline of the Genoese economy in the eighteenth century.

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T. R. RAVINDRANATHAN. *Bakunin and the Italians*. Kingston, Ont.: McGill-Queen's University Press. 1988. Pp. x, 332. \$35.95.

This excellent book is partly a biography of Michael Bakunin and partly a history of early Italian socialism. As biography, it fills a gap in the literature. The standard biographies of the Russian anarchist, notably those written by E. H. Carr and Ju. M. Stekloff, devote minimal attention to the Italian period (1864–67) of Bakunin's career and virtually ignore his influence on the Italians. In contrast, T. R. Ravindranathan examines all major aspects of Bakunin's association with Italian radicals, covering the period from his arrival in Florence in 1864 to the anarchist insurrection at San Lupo in 1877, which was conducted after his death but in accordance with his tactics. Thankfully, Ravindranathan does not indulge in the Bakunin-bashing that has become so fashionable in recent years. Although he does not hesitate to note the Russian's ideological inconsistencies and personal failings, Ravindranathan portrays Bakunin as a serious and devoted revolutionary, an acute thinker capable of extraordinary insights into the Italian and European situations, and a master propagandist who successfully undermined rivals such as Giuseppe Mazzini, Giuseppe Garibaldi, and Friedrich Engels.

As a study of early Italian socialism, the book covers

only those developments in which Bakunin was directly involved or on which he exercised a decisive influence: the first anarchist group in Naples, the destruction of Mazzini's apostolate, the growth of the First International in Italy, the fight against Marx and Engels and the General Council of the International, the rise and ideological orientation of the Italian Federation, and the insurrections of 1874 and 1877. All of these themes have been treated previously in the works of Aldo Romano (1954) and Richard Hostetter (1958), among others. Nevertheless, although it does not explore new territory, Ravindranathan's tighter and more readable narrative, based on extensive study of the sources in Russian as well as in Italian and French, provides a worthy supplement to these older works. The author's principal contribution is his reexamination of several important controversies regarding Bakunin's role in the history of early Italian socialism. The end result is a more positive assessment of Bakunin's involvement with the movement than that provided by either Romano or Hostetter, especially the former.

In the final analysis, Ravindranathan's conclusions are not dissimilar from those of Hostetter. He considers Bakuninism in Italy to have been a failure, because it did not have a lasting influence comparable to Marxism in later years. He notes, however, that, just as populism gave way to Marxism in Russia, only after the failure of Bakuninism was Marxism able to penetrate Italy in the 1880s. "The significance of the Bakunist experiment," therefore, "lies in the fact that it first overcame the bourgeois republicanism of Mazzini and then became the first socialist movement to organize a popular opposition to the establishment" (p. 237).

One wishes that Ravindranathan's analysis of the "failure" and legacy of Bakuninism in Italy had been more extensive. His discussion of the "disintegration of Bakuninism" is disappointingly brief, with some surprising omissions—for example, the adoption of anarchist communism—that are not explained. Although justified in treating the anarchist insurrection of 1877 as an example of Bakunin's belief in the revolutionary efficacy of insurrectionary shock waves, Ravindranathan does not adequately explore the relevance of "propaganda of the deed," the theory that Carlo Cafiero and Errico Malatesta—the leaders of the 1877 insurrection—articulated in 1876. And Ravindranathan's tendency to regard Bakuninism and Italian anarchism as synonymous, even as late as 1877, is questionable. By that year, Cafiero, Malatesta, and other leaders had abandoned Bakunin's conception of anarchist collectivism and espoused the anarchist communism later associated with Peter Kropotkin. As Malatesta once explained, the Italian anarchists were Kropotkinists before Kropotkin.

But these minor flaws do not detract from Ravindranathan's sound scholarship or the importance of his contribution. Hereafter, the book should be considered

required reading for all students of Bakunin and early Italian socialism.

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ALEXANDER DE GRAND. *The Italian Left in the Twentieth Century: A History of the Socialist and Communist Parties*. Bloomington: Indiana University Press. 1989. Pp. xiv, 182.

JOAN BARTH URBAN. *Moscow and the Italian Communist Party: From Togliatti to Berlinguer*. Ithaca: Cornell University Press. 1986. Pp. 370. Paper \$14.95.

Italian communism seems to hold a particular fascination for American scholars. Perhaps the size of the Italian Communist party (PCI) combined with its maverick character and the incredibly complex context in which it operates help explain this attraction. Usually neglected in English-language works, however, is the interrelationship with socialism, a movement that, despite Communist rhetoric, supplied the tradition and many of the ideas for what remains most viable, attractive, and appealing in Italian communism.

It is thus a pleasure to note the appearance of Alexander De Grand's book, which supplies the English-speaking reader with a history of both parties, beginning with the foundation of the Italian Socialist party (PSI).

Unlike Joan Barth Urban's work, De Grand's short, crisp book introduces an important topic rather than tackling in detail a narrower research project. The book's scope makes it an excellent general introduction for undergraduates studying late nineteenth- and twentieth-century European leftist movements and an ideal starting point for graduate students interested in the same area. As in his earlier synthesis of Italian fascism, De Grand does a remarkable job of condensing a very complex history into a manageable story. Scholars might have urged his publisher to have allowed him more pages to discuss at greater length and to unpack further the ideology and significant events of his tale; this fuller explication might have made the schisms, splits, divisions, and wrangling seem less futile and confusing than they may perhaps appear to readers unfamiliar with Italian politics.

By providing an almost side-by-side summary of the thought and action of both parties, however, De Grand renders a unique contribution. Despite the twists and turns of both movements, the reader is able to judge, at least by implication, how close the movements have been in the past and may be moved to inquire whether the long-term effects of the communist split from the socialists were in fact pernicious.

The question may be put because, as De Grand writes in his introduction, "I believe that reformism was the only realistic possibility for the proletarian movement during the period covered in this book" (p. xiii). After describing the developments of the previous hundred years, he repeats the same concept in his

conclusion, whereas Urban states in her introduction, "One of my central arguments is that the PCI leaders from Togliatti to Berlinguer posited reformist alliance policies that, while not breaking with the capitalist order, would—so they hoped—facilitate an eventual transition to socialism" (p. 16). This statement simply reiterates the thinking of early socialist leaders such as Filippo Turati. Until recently, however, the PCI denounced reformism and split from the PSI primarily because of the policy. So how should we judge Urban's statement?

Here De Grand helps. He lays out the arguments between socialist reformists and revolutionaries (maximalists). In effect, the socialists lurched from one policy to the other, unable to decide between either, because both policies emerged from reasonable readings of Karl Marx. The Communists circumvented the dilemma by, in effect, suppressing discussion about the opposing theories of achieving socialism. Instead of becoming immobilized, the PCI repressed dissidents and expelled members who would not conform to the party line. Urban details very well how frequently the PCI did this at the behest of Moscow, in altered international conditions; even Palmiro Togliatti—long-time PCI leader most frequently identified with Soviet domination—had autonomist sentiments but, to save himself and his party, had to go along with Joseph Stalin's dictates. Moreover, "democratic centralism" stifled open debate within the PCI until recently. An image of a decisive PCI emerged in contrast to a squabbling PSI.

The PCI also used another technique, the "double track" policy, to cover up the inherent contradictions in Marx. The PCI always declared itself to be a revolutionary party, thus promising its radicals the millennium, but, at the same time, instituted a mass party that would come to power by reformist social democratic means. The "*svolta di Salerno*," by which the Communists suddenly supported the undemocratic Badoglio government after the fall of Benito Mussolini, initiated this cynical policy. Thus, the PCI piled up votes based on its usurpation of the socialist tradition, while the faithful awaited a revolution that never came. In the end, this "elaborate hoax" turned out not to be on the capitalists. "Rather," De Grand correctly states, "Togliatti and his successors played their double game against their own membership in calling for revolution but following reformist policies" (p. 164). Socialists may have squabbled and split, but the dialogue produced ideas that became implanted within the national fabric and account for some of the best ideals of Italian society.

Instead, Communist *doppiezza* has left a lingering legacy of distrust, not only in Italy but also on the international scene, despite the sympathy with which many foreign scholars view the PCI.

In her preface, Urban notes this continuing unwillingness of the Western diplomatic community to issue the PCI formal democratic credentials. In her study, Urban provides an exact historical reconstruction, ar-

guing that serious differences of opinion between Italian and Russian Communists existed from the beginning. She also chronicles the course by which the PCI has apparently achieved autonomy from, and become critical of, the Soviet system, so much so as to have issued a "challenge to Moscow."

Events since the book's publication have accentuated this tendency, with Secretary Achille Occhetto proclaiming a "new" Communist party more akin to West European social democratic organizations than Communist ones and pushing for a change of name. But good intentions and an understanding of the reasons why the Italian Communist leadership engaged in repressive policies are not enough to sweep away sixty years of mistaken policies and prognostications. True, the PCI has changed, but its changes appeared remarkable only when compared with the glacial immobility of other communist parties. The PCI now formally accepts social democracy, NATO, and European cooperation, but it reached those conclusions belatedly, after interminable hedging, after having initially condemned all of them on suspect grounds, and only after having been forced to do so by international events and the pressure of its middle-class electorate.

It will be interesting to watch how evolving international developments will influence the PCI's future course. Since Italian voters have always rewarded PCI demonstrations of autonomy toward Moscow, there is a good incentive for continued independence, and both the European Community and Mikhail Gorbachev's fate will play an important part. Within Italy, the legacy of distrust may eventually fade, but for now it remains strong.

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CHRISTOPHER DUGGAN. *Fascism and the Mafia*. New Haven: Yale University Press. 1989. Pp. xiii, 322. \$27.50.

As the title indicates, this book brings together the two themes of modern Italian history that have most fascinated both scholars and the general public in the twentieth century. Readers, however, should not expect to find titillating new details about the rites and rituals of an all-powerful criminal conspiracy. On the contrary, what Christopher Duggan provides is a work of explicit demystification that challenges popular notions about the nature of the Sicilian Mafia and the success of fascism's anti-Mafia campaign in the 1920s. In the process he helps revise our understanding of the Mafia, the Sicilian fascist movement, and the role of Benito Mussolini's regime in southern Italy.

The book divides into two rather distinct parts. In the first, Duggan advances his thesis that the Mafia as a single well-structured organization is largely a myth. Although this interpretation is not especially original, he does offer an engaging explanation for the extraordinary persistence of such a myth over the last century.

In his view the concept of the Mafia as a great under-world conspiracy took hold in the second half of the nineteenth century primarily because it served the needs of a variety of interests within Sicily, the state apparatus, and the national political arena. Faced with serious political unrest, social tensions, crime, and *omertà* after unification, representatives of the new Italian state embraced a conspiratorial vision of the Mafia. Not only did it offer a convenient explanation for confusing and complex aspects of the Sicilian problem, but it also justified intervention by the army and police, which suited the authoritarian cast of the largely northern Italian officials. The concept proved no less useful to the landed elites, because it permitted them to label popular social movements, such as the Sicilian *fasci*, as criminal conspiracies that required repression, not reform. Conversely, Italian socialists wanted to see the Mafia as a creature of the *latifondisti*; the defeat of the Mafia thus would entail sweeping land reform to break the power of its aristocratic patrons.

The second part of the book explores the confrontation between fascism and the Mafia by focusing on Mussolini's "prefect of iron," Cesare Mori, and his famous campaign against the Mafia. Based on exhaustive research in Mori's voluminous papers, these chapters represent the most original and valuable part of the study. Duggan shows clearly how the prefect obtained his seemingly impressive results by resorting to hostage taking, vague charges of "criminal association," indiscriminate mass arrests, torture, and show trials that sacrificed defendants' rights for quick convictions. He concludes that such methods did more harm than good. Mori identified the social core of the Mafia with the rural middle class who accounted for most of those arrested in the campaign of 1925–29. In this way he attacked a segment of society that most threatened the dominance of the *latifondisti* and thereby helped bolster their power, largely at the expense of the peasants. Duggan's analysis suggests that fascism's impact in the south was quite different from its impact in the northern countryside where the regime favored the advance of new agrarian business interests over the old absentee landlords.

Duggan's rigid defense of his general thesis on the myth of the Mafia detracts from his intelligent and insightful treatment of the fascist response to the Sicilian problem. What emerges is a curiously static portrait that gives the impression of little or no change in Sicilian society, the Mafia, and governmental policies over the last one hundred years. Although Duggan is good at telling us what the Mafia was not, he is much less effective at telling us what it was. For that, readers would be better advised to consult the works of Henner Hess, Anton Blok, and Raimondo Catanzaro.

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ILIJA MITIĆ. *Dubrovačka država u međunarodnoj zajednici: od 1358 do 1815* [The Dubrovnik State in the International Community: From 1358 to 1815]. (Nakladni Zavod Matice Hrvatske.) Zagreb: Jugoslavenska Akademija Znanosti i Umjetnosti. Pp. 299.

Ilija Mitić has written a monograph directed at general readers on the Republic of Ragusa (Dubrovnik) from 1358 to its fall in 1815. It is a compact study, 240 pages in length, with maps, photos of documents, and illustrations as well as a résumé in English. It puts forward the forthright claim that Dubrovnik warrants consideration among the European states of early modern times. Mitić emphasizes the components of the state that established Dubrovnik's autonomy, that is, its long history as a self-determined and recognized member of the European state system. His earlier study on the unique consular service of Dubrovnik, published in 1973, plays a significant role in proving his case because Mitić identifies the consulate as a vehicle for the execution of an independent foreign policy. Ways and means of statecraft receive so much emphasis in the study that the reader may be puzzled as to why the author expends his effort proving Dubrovnik's autonomy and role among the Mediterranean powers. But, unfortunately, historians of Dubrovnik find difficulty in moving their interpretations beyond this point because the general audience that once recognized Dubrovnik as a successful aristocratic state no longer exists. The models that dominate our understanding of European state development no longer hold a place for a community with Dubrovnik's characteristics.

Two reasons for this state of affairs deserve mention. First, Dubrovnik's history has been written with great pride in the national tongue, Serbo-Croatian. Other small European states have foregone this assertion of cultural identity in order to reach a wider audience, and they publish in more generally read languages. Dubrovnik's central role in Yugoslavian national formation precludes this choice, so inaccessibility has marked the decades that have produced Dubrovnik's corpus of critical historical studies. The résumé in English at the end of this study is an attempt to rectify this situation. Second, an even more serious dilemma lies in Western thinkers' skepticism that a "pure" aristocracy such as Mitić describes in these pages ever existed within the early modern European state system. Twentieth-century Venetian historiography has gone a long way toward disabusing us of a belief in successfully functioning aristocracies. As Mitić asserts frequently, however, Dubrovnik was not Venice, nor was it dominated by Venice after 1358 or a slavish imitator of Venetian policies.

Mitić's monograph accurately reflects the concerns of Dubrovnik historiography, and perhaps his work deserves reading on that account. Others make attempts to reach general readers through publishing documentary evidence, a project long abandoned in most other European states. Josip Lučić has recently reedited the earliest extant charters from the thirteenth century,

and Branislav Nedeljković published the *Liber viridis* (statute law) in 1984. Is a Western audience ready to accept Mitić's analysis and the published documentary evidence supporting it: asylum granted to foreign exiles (sometimes belligerents from opposing sides), a constantly updated military arsenal left untried in battle, centuries of neutrality among belligerent neighbors, social stability at home, and prosperity on a scale matched by few contemporary powers? Mitić provides impressive proofs for these claims; whether readers will adjust their understanding of European state formation to accommodate this exceptional city-state is another question.

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ELEMÉR ILLYÉS. *Ethnic Continuity in the Carpatho-Danubian Area*. (East European Monographs, number 249.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1988. Pp. xiii, 439. \$55.00.

The rather innocent title of this study does not reflect the fact that Elemér Illyés's main concern is the historiographic controversy of the past two centuries or more concerning the historical rights of the Romanians and Hungarians to Transylvania. The controversy revolves around the so-called theory of a Daco-Roman-Romanian ethnic or political continuity in Transylvania.

The study is divided into four chapters that vary in length: "History" (66 pp.); "Archaeology" (128 pp.); "Linguistics" (100 pp.) and "Geographical Names" (45 pp.). The substantive chapters are followed by notes (63 pp.), selected bibliography (20 pp.), and indexes of names associated with the history of the region and of authorities cited (20 pp.). A gazetteer (7 pp.) and a subject index (2 pp.) conclude the volume.

In a brief introduction, Illyés notes that, for his arguments, "the most reliable knowledge has been gleaned from Balkan philology," while "archaeology is not always reliable" and written records are "scanty and fragmentary" (p. xi). In my view neither archaeology nor philology will suffice to solve historical controversies without a basis in concrete facts. For a historian, political or ethnic history can be reconstructed only from written historical evidence. The fewer the written materials, the more attention that should be paid to an in-depth analysis of what material one has.

The theory of a Daco-Roman-Romanian continuity that underlies the Hungarian-Romanian political controversy is based on very few facts, and even those are hard to verify. The scholastic controversy consists of conflicting views about the reliability of one or another medieval author who happened to mention the name "Vlach" (*Blachi*, *Blachii*, *Blasi*, *Blasii* and so forth), the name also used for the predecessors of the Romanians.

The main source that uses the terms *Blachii*, *Blacus*, *Blaci* is the *Gesta Hungarorum* of Hungarian Anonymus

Notarius Belae regis (thirteenth century). He placed the *Blasi* in Transylvania at the time of the arrival of the Hungarians (ca. 895). As a result, the *Gesta* of Anonymus became the cornerstone on which the theory of the Daco-Roman-Romanian continuity is built, while the opponents of the theory declared the *Gesta* to be a fiction, an invention by Anonymus.

The proponents of the theory of continuity have so far been unable to prove that Anonymus was right, while the opponents of the theory are equally unable to prove him wrong—hence, the controversy. The reader of this book may well expect to find some persuasive arguments in favor of one or the other school of thought.

Disappointingly, not only is the historical chapter the shortest, but the treatment of the few facts contained in sources is unsatisfactory. Although Illyés proceeds to quote in English the relevant chapters from the *Gesta* of Anonymus, he does so on the basis of the Hungarian translation rather than from the Latin original. As a result, "terra Ultrasilvana" (for Transylvania) becomes a less specific "territory beyond the forest." Although the author gives his translation the heading "from chapter 25–27" (p. 17), he omits—one hopes inadvertently—chapter 26, in which the crucial statements used by the proponents of the theory of continuity, such as "Gelou dux Blacorum" and "Gelou vero dux Ultrasilvanis," are found. Unconvincing is Illyés's view that "Anonymus wrote a narrative to demonstrate the courage of the [Hungarian] landowners' ancestors, and for this needed fearless leaders [like Gelou] whom he simply invented" (p. 19).

The chapter dealing with historical sources and historiography devotes two pages to the writings of "Transylvanian German (Saxon) historians of the seventeenth to eighteenth centuries on the origin of the Romanians" and twenty-two pages to Romanian national, Romantic, and Marxist historiography. Illyés fails to offer a corresponding evaluation of the parallel stages of Hungarian historiography on the same subject, although many Hungarian historians are guilty of misplaced patriotism in the past and even today, as are their Romanian counterparts.

As in the case of the treatment of written sources, the entire narrative of the author in the chapters on archaeology and philology consists of juxtaposing opposing arguments used by modern authorities in these respective fields of scholarship. Although Illyés leans toward one school of thought, namely the one denying a Daco-Roman-Romanian continuity, the only lesson to be drawn by a historian from this study is that artifacts and phonetic changes may confirm attested historical facts but will not substitute for a lack of them.

The set task of Illyés, to explore the problems of "ethnic continuity in the Carpatho-Danubian area," did not yield the expected result. Nevertheless, the reader receives a formidable survey of past and current historiography of the subject presented in the text and in over eleven hundred footnotes. Some of these notes provide a brief annotated bibliography that may serve

as an introduction to research on specific issues. One also learns from the footnotes that the author derived his negative evaluation of Anonymus concerning the Vlachs from the works of Gy. Györffy (see the footnotes on pages 340–42). Upon checking, however, it appears that Györffy's views on crucial issues are not footnoted with references to sources. Valuable also is the bibliography consisting of some four hundred items.

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CORNELIA BODEA and HUGH SETON-WATSON, editors. *R. W. Seton-Watson și românii, 1906–1920* [R. W. Seton-Watson and the Romanians, 1906–20]. In two volumes. Bucharest: Editura Științifică și Enciclopedică. 1988. Pp. xxv, 566; 576–999. 122.00 L the set.

The memory of R. W. Seton-Watson is still greatly revered among many peoples of Central and South-eastern Europe, especially the Yugoslavs and the Romanians. It was he more than any other person who brought to the attention of Europe the mistreatment of national minorities in portions of the Habsburg monarchy ruled by Hungary. In the decade before World War I, Seton-Watson traveled extensively in Austria-Hungary and adjacent lands, acquainting himself with conditions there and developing close friendships with many nationalist leaders. His publications resulting from these investigations were a devastating critique of Hungarian racial policies. Initially, he favored the maintenance of the Habsburg empire but in a federalist structure with equal rights for all nationalities. Like many of his nationalist friends, Seton-Watson pinned his hopes for reform on the accession of the Archduke Francis Ferdinand. But the assassination of the archduke in Sarajevo and the outbreak of war convinced him that the maintenance of the Austro-Hungarian monarchy in its existing form was no longer feasible, and he began to work immediately for the creation of a Yugoslavia and a Greater Romania. During the war, Seton-Watson served as an influential advisor to government leaders in England, as an intermediary with nationalist leaders, and as the editor of *The New Europe*, a periodical that championed the creation of the successor states. The treaties that emerged from the Paris Peace Conference created the essence of what Seton-Watson had worked for, and, in recognition of his contribution, he was feted by Yugoslavia and especially Romania. In 1920, he was honored at a remarkable session of the Romanian parliament.

The volumes under review are the most recent of several publications that document and put into context Seton-Watson's career. In 1976, Yugoslav historians with the cooperative editorship of his distinguished sons, Christopher and Hugh, published twin editions of his Yugoslav correspondence (*R. W. Seton-Watson and the Yugoslavs: Correspondence 1906–1941* [1976]; and *R. W. Seton-Watson i Jugoslaveni: Korespondencija 1906–*

1941 [1976]). In 1981, Christopher and Hugh published a monograph based on their father's private papers and published writings (*The Making of a New Europe: R. W. Seton-Watson and the Last Years of Austria-Hungary* [1981]). Now the Romanian counterpart of the Yugoslav volumes has appeared, edited by Hugh Seton-Watson and Cornelia Bodea. The Romanian and Yugoslav volumes reproduce many of the documents on which *The Making of a New Europe* was based. All three publications can be used together profitably.

In addition to Seton-Watson's correspondence to and from Romanians or about Romania, these volumes under review also include historical notes and other unpublished materials from his personal papers, selections from his published works, and excerpts from Romanian publications. The documents are published in their original languages with English, German, and French predominating, as Seton-Watson's Romanian friends often wrote in these languages. These volumes are especially accessible to the non-Romanian reader by the presentation of the entire critical apparatus in both English and Romanian. This apparatus includes introductions by both Hugh Seton-Watson and Bodea, unusually extensive document summaries, and detailed footnotes. Because of the untimely death of her collaborator in 1984 and because of the nature of the material, Bodea has borne the major share of the herculean labor of editing and annotating these volumes. The footnotes, especially, provide a rich fund of hard-to-find biographical information and excerpts from additional documents in Romanian archives. Bodea's meticulous work includes an extremely comprehensive index of more than forty pages that gives the reader instant access to specific issues and persons.

These volumes will be of most interest to those concerned with the history of the Habsburg monarchy, the evolution of the Romanian state, and the development of British war aims during World War I. In addition, they provide a perspective for all those interested in the current issues of nationalism in East Central Europe.

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JOHN S. KOLIOPOULOS. *Brigands with a Cause: Brigandage and Irredentism in Modern Greece, 1821–1912*. New York: Clarendon Press of Oxford University Press. 1987. Pp. ix, 342. \$68.00.

Brigandage in Greek society has received too little attention as an object of comprehensive study and too much attention in one-sided preoccupation with its role in the emergence and expansion of the Greek nation-state. Its practitioners have been elevated to the status of superpatriots or "social bandits" engaged in armed resistance against the Ottoman colonial power and its domestic collaborators, Greek as well as Turkish. In undertaking the ambitious task of reversing that histo-

riographical trend, John S. Koliopoulos has produced a study that is at once ground-breaking and revisionist.

Though confined to nineteenth-century brigandage on both sides of the boundary between Greece and the Ottoman empire, this book encompasses the entire class of men trained in the traditional methods of guerrilla warfare, "military enterprisers" who played an influential role in the Greek state, and their traditional *armatolik* system, which reconstituted itself after independence in ways that the Greek state was forced to accommodate. Because *armatolism* entailed the use of brigands to fight brigands and a frequent shift from one side of the law to the other, this broad definition of subject is justified and demands the equally broad treatment that it receives. Koliopoulos uses a vast range of published primary sources and secondary works and original research in state and private archives. He addresses two basic questions: why brigandage and the traditional military class were able to withstand the onslaught of Western-style institutions inimical to them and what impact their survival had on the pace and character of modernization. He answers the first by showing how much they were nourished by animal husbandry at the socioeconomic level and state-mediated irredentism at the politico-ideological level and by examining the tension in official policy between repression and cooptation. In response to the second, he demonstrates how skillfully the military manipulated new institutions to serve their own interests, how corrosive to civil liberties legislation against brigandage could be, and how easily, during moments of irredentist upheaval, the regular army could dissolve through defections from its ranks. He contends, therefore, that the persistence of brigandage and *armatolism* obstructed and delayed modernization and on occasion less persuasively suggests that it divested the modernization process of any function but that of giving traditionalism new life through a new set of forms and a new badge of legitimacy.

Though outstanding in its achievements, this study is not without weaknesses. Organizationally, it is bifurcated between a core of dense sequential analysis, which will swamp some general readers, and a collection of topically defined and chronologically overlapping essays of greater suitability for a general audience. The essays, however, are sometimes polemical in tone and facile in generalization.

Second, Koliopoulos tends to replace the superpatriot distortion of romantic nationalists with his own, namely, the predator whose only cause was self-interest or self-preservation. Though never directly affirmed, this counterimage is strongly suggested by the absence of effort to cite disinterested and salutary military behavior, however infrequent; by a tendency to rule out the possibility that one can be both public-spirited and self-interested; and by making insufficient allowance for the fact that the written record, based largely on hostile or ambivalent sources, focuses on moments of unsuccessful irredentism, when traditional military behavior was likely to be at its worst.

Third, revisionism sometimes lapses into rejectionism. How, for example, can one know that peasants, who left no written record, cooperated with brigands out of fear rather than from sympathy or both? Can one dismiss the favorable assessments of folklore and nationalist history as wishful thinking without eliminating the evidence that can correct a largely negative picture implicit in the official record?

Fourth, the role of military enterprisers in obstructing, delaying, and corrupting the modernization process tends to be exaggerated. This tendency is counteracted by a masterful analysis of general conditions inimical to modernization. Without comparison, however, between the behavior of military enterprisers and that of other elite groups, one cannot adequately assess military influence in shaping the course of development or know whether the qualities on which the image of predator is based were distinctively military rather than shared with elite groups of even "modern" complexion.

Finally, military enterprisers look more like agents than objects of history, and their nature appears more static than it probably was. Koliopoulos fails to address a third basic question: how modernization affected the military class. His analysis reflects a flaw common to the modernization model that he employs: treating the traditional military class as a relic. Had this class and its infrastructure been conceived to reflect nineteenth-century conditions as well as those of a bygone era, he might have found a more fruitful way of conceptualizing their relationship to modern nationalism. He might have considered the possibility that traditional military behavior reflected not the absence of nationalism but the gradual process of being nationalized and the emergence of understandings of "nation," "state," "freedom," and "authority," distinct from but influenced by what the state and other groups were defining as canon.

To ask so much of a single study with so much to tackle may seem to smack of demanding the impossible and underestimating the value of what has been done. Let it be said, therefore, that one can ask for so much only because of what this study has achieved. Precisely because this subject will require the cumulative efforts of future historians, one wishes that Koliopoulos had tried to be more questioning and less definitive so as to set an informed research agenda for the future.

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PETER J. STAVRAKIS. *Moscow and Greek Communism, 1944-1949*. Ithaca: Cornell University Press. 1989. Pp. xvi, 243. \$28.95.

The thesis of this monograph is that the Greek Communists ruined their chances of attaining power in Greece after World War II largely because they deferred to advice and instructions from Moscow, where the interests of the Soviet Union rather than the

interests of the Greek Communist party prevailed. Despite the fact that Peter J. Stavrakis derives most of his information about Soviet policy in Greece from tendentious memoirs written by Greek and Yugoslav Communists to explain away the defeat they suffered in the civil war of 1947–49, he makes his case convincingly.

Other scholars have come to the same conclusion before him. This book, therefore, brings the information up to date by taking account of all of the testimony and scraps of information (and misinformation) that have appeared sporadically in the Greek press and in books of memoirs. Stavrakis is a careful critic of his sources and weaves them together into a thoroughly plausible, although admittedly incomplete, account of Greek Communists' relations with Russians from Stalin to the present.

Nevertheless, the book is awkwardly put together. Stavrakis repeats himself systematically, but the reader has to wait for the appendix to find out why. There he distinguishes mere historical narrative from the loftier clarity of social science and makes it clear that his goal as a political scientist is to attain the higher plane through what he calls "path analysis." This requires him first to examine the historical record closely; then "analysis begins with the description of a set of general variables that can be assumed to exert a significant effect on the course of Soviet policy" (p. 218). "Next, the exogenous factors have to have meaning in the specific context. This is established by selecting from the historical record a set of propositions on which to base an interpretation of Soviet policy in Greece from 1944 to 1949" (p. 218). This, in turn, permits him to "conclude which tactical alternatives and policy instruments the Soviets actually employed, and with what success. Similarly, I can determine the validity of the 'principles' commonly assumed to guide Soviet foreign policy. This kind of analysis permits me . . . to evaluate larger patterns of Soviet international behavior" (p. 219). Thus, scientific truth emerges from the "multiple and conflicting interpretations" (p. 217) of mere historical narrative but only after the reader has been led over the same chronological ground two or three separate times.

Stavrakis's pretentious methodology sets up a straw man, what he calls "the common error of viewing Soviet policy as monolithic" (p. 219), and then demolishes it with gusto. But, to a mere historian like myself, it seems a wasteful and sometimes even silly exercise, repeating without improving on points already made in his initial historical narrative.

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J. ROBERT WEGS. *Growing up Working Class: Continuity and Change among Viennese Youth, 1890–1938*. University Park: Pennsylvania State University Press. 1989. Pp. ix, 206. \$24.00.

This somewhat short but intriguing study of young people growing up in Viennese working-class society is a real contribution. Page after page of this book is a welcome and, at the same time, an intellectually refreshing effort. It is so appealing because it is free of the ideological biases and contortions that have blurred the study of European labor history up to this point.

In his investigation of working-class youth between 1890 and 1938, J. Robert Wegs is able to break down previous Marxist preconceptions that viewed the proletariat as a unified group with a single set of aspirations. Instead, with sophisticated effort he understands that working-class society was subdivided into at least three groups: skilled, semiskilled, and unskilled. Wegs explains to his readers that these three strata really had their own distinct cultures. These telling insights are the result of his viewing the various working-class cultures from the "bottom up" through oral interviews that he and others judiciously conducted with those who had grown up within working-class cultures in the time period mentioned.

Wegs's major theme, and it is a very convincing one given his evidence, is that the values of working-class youths were not shaped by distant, outside influences. They were not, it seems, indoctrinated by either bourgeois society or the ideas of the Socialists. Instead, their attitudes were the result of their own immediate environment. According to Wegs, there were five factors involved: family, gender, strata in working-class society, economic conditions, and the scarcities with which they grew up.

These working-class children and youth were essentially immigrants. They were the offspring of German and Czech migrants from Bohemia and Moravia who largely formed Vienna's new factory and artisanal elements beginning in 1890. The life style of these new arrivals to Vienna was the result, according to Wegs's evidence, of a number of trying conditions that they had to face. To begin with, there was the sheer size of their families, averaging 5.18 children per family. Beyond this, they were pressed into apartment buildings that averaged 1.36 persons per room, with the situation far worse in distinct working-class districts. Crowding of this dimension, Wegs tells us, produced a kind of culture of the hallways and streets. These two areas became the centers of community life for Viennese youth within which personal problems became public knowledge and working-class cultures developed through verbal reinforcement.

As this time period wore on, conditions improved for these Viennese youths thanks to the development of the economy. Family size got smaller, housing improved, and privacy became a reality. Proletarian rejection of schooling dissolved into acceptance. With more education, working-class boys could move into higher-paying skilled positions, and working-class girls could aspire to white-collar jobs.

Wegs's study proves rather convincingly that poverty is not necessarily corrosive. It is amazing how many proletarians made something positive out of a bad

social and economic beginning. If Wegs's superlative analysis demonstrates anything, it is that historians need to listen more to those workers who survived these conditions and less, it would seem, to the preconceptions of those in the business of writing history.

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TEODOR MATS'KIV. *Het'man Ivan Mazepa y zakhidn'o-evropeiskyykh dzhherelakh, 1687–1709* [Hetman Ivan Mazepa in Contemporary West European Sources, 1687–1709]. Foreword by LIUBOMYR VYNAR. Summaries in English, French, and German. (Ukrains'kyi vil'nyi universytet, Seriiia monohrafi, number 45.) Munich: Ukrainische Freie Universität. 1988. Pp. 286.

The express purpose of this study is to shed light on the person and issues behind the elusive and still controversial leader of Cossack Ukraine, Hetman Ivan Mazepa, who joined Charles XII of Sweden against Tsar Peter I. It reviews and analyzes contemporary references and accounts of Mazepa in German, English, Swedish, Dutch, Austrian, Swiss, and other sources, including memoirs, correspondence, diplomatic communiqués, and periodical press. Some interesting archival materials, notably Austrian and English, are also included. Although most of the chapters adhere to a logical order (for example, "Mazepa in French Sources"), there are such seemingly disparate chapters as "The Battle of Poltava," "Mazepa—Author of the Ukrainian-Swedish Alliance," and "Legend and Truth about Mazepa." Finally, there are pertinent bibliographies, seventeen appendixes, and twenty-eight illustrations complementing the text.

This exhaustive study of extraneous commentaries is quite authoritative and represents a significant contribution to the still inadequate Mazepian studies. Nevertheless, it is also a victim of the inherent limitations of the sources. Clearly, contemporary newspaper accounts and private memoirs, mixed with occasional diplomatic reports, are of limited value for the modern scholar in that they serve mostly as a barometer of the professed contemporary public and official opinion. Thus, one is left with the pervasive impression that Mazepa, at least prior to the debacle at Poltava, enjoyed great renown and admiration among his contemporaries. Were the study limited to mere factual description and analysis of the available sources, it would prove quite satisfactory. Alas, it goes beyond its logical scope by seeking to vindicate Mazepa's policies through the eyes of foreign observers. Throughout there is an unfortunate tendency to juxtapose contemporary impressions of Mazepa with modern scholarly interpretations in order to validate the nobility and the rationality of his famous "betrayal." There is also much unfortunate shadowboxing with various Russian and Soviet historians, who persist in the vilification of the venerable Cossack leader intent on breaking free from Petrine

regimentation—all despite the concluding observation that "today it is no longer necessary to defend the policies of Mazepa" (p. 186). Yet this is precisely what this work, stridently at times, does.

In the end, the work demonstrates a superb command of sources and meticulous detail, but it is weakened by inadequate editing and profusion of redundancies. Even more crucial is the absence of a central focus or theme to redeem the valiant effort. It borders on pastiche, a frustrating amalgam of pertinent facts, marginalia, and subjective opinion that further mystifies Mazepa's historical persona. The central issue animating Mazepa's fateful decision, much less the inherent drama within the man himself, lies beyond this effort.

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ZENON E. KOHUT. *Russian Centralism and Ukrainian Autonomy: Imperial Absorption of the Hetmanate, 1760s–1830s*. (Harvard Ukrainian Research Institute, monograph series.) Cambridge: Harvard University Press, for the Harvard Ukrainian Research Institute, Cambridge. 1988. Pp. xv, 363. \$19.95.

The publication of Zenon E. Kohut's valuable study on the integration of the Ukrainian Hetmanate into the imperial system is an event to be welcomed by anyone interested in the formation of the Russian empire. The author first declares Russia to be a unitary state challenged from the beginning of the eighteenth century by claims of autonomy in the Ukrainian and Baltic areas. He then traces the social history of the Hetmanate until the abolition of the Hetmancy in 1764 and discusses the influence of Western ideas on Catherine II's policies. He finds that these ideas strengthened the traditional belief in the superiority of the unitary state over any form of contractual relationship between monarch and subjects. In the fourth chapter the author examines the early years of P. Rumiantsev's governorship, and in the fifth he discusses the Legislative Commission of 1767–68 and the varied responses of the gentry, Cossacks, clergy, and townsmen to Ukrainian autonomy and the prospects of Russian interference in the affairs of the Hetmanate. This chapter, the longest in the book, tends to be overly descriptive; a detailed analysis of the *nakazy* (legislative instructions) would have enabled the author to present his views on the controversy more persuasively. The next chapter covers the formal introduction of the local government reform of 1775, and the last, which I think is the best, deals with the adjustment of the various segments of Ukrainian society to the imperial order. Readers will follow with interest Kohut's analysis of O. Bezborodko's views and of the two attitudes toward integration, which he calls the assimilationist and the traditionalist. A comprehensive bibliography of published sources concludes the volume.

Kohut strongly hints at the beginning that the Ukrai-

nians of the Hetmanate were the innocent victims of Russian aggression against their "rights and privileges," although we are told toward the end that many Ukrainian nobles found their incorporation into an imperial nobility a profitable outcome of that aggression. This thesis and the contradiction cannot be left unchallenged.

We are never given a systematic exposition or definition of those famous "rights and privileges" of the Ukrainian gentry. Perhaps Kohut agrees with the Ukrainian and Baltic nobles who felt that the issue was a Pandora's box around which it was politically expedient to dance but dangerous to open. Had he opened it, he would have had to pay more attention to the land question and to the degradation of the Cossacks.

It was not the Russians who abolished one of the most important "rights and privileges" of the Cossacks: the right to elect their leaders. Kohut concedes that much. He recognizes the rapacity of "officers and notables" and their pressure under which the "Cossacks lost all their political prerogatives," including the right to elect their officers (pp. 33, 64). If that was the case, why attack Rumiantsev (p. 114) for abrogating "their most ancient right of selecting their leaders"? The degradation of the Cossacks cannot be viewed as a direct result of Russian centralism and cameralism, the two main offenders in Kohut's book. He neglects to cite the figures contained in a document presented to the Senate in 1763 by Kyrylo Rozumov's'kyi showing that almost all of the lands of the Cossack state had by 1750 become the properties of the Hetmanate's nobility. As a result, the Cossacks were indeed rendered "hardly distinguishable from the peasants" (p. 33), while the peasants had become "virtually enserfed" (p. 37) when the last Hetman in 1760 restricted "almost completely" their opportunities to move (p. 38). What had happened to the "rights and privileges" Bohdan Khmel'nyts'kyi had so valiantly fought for in the 1640s?

While this degradation and impoverishment were taking place, the Hetmanate's landowners were doing well and were ready at the Hlukhiv (Glukhov) council of 1763 to call for the confirmation of their holdings and the freezing of the social order. They also asked for the rights and privileges already enjoyed by the Russian nobility, and one of their spokesmen, Hryhorii Poletyka, even presented in 1768 a projected charter of rights and privileges strongly reminiscent of those of the Polish nobility (pp. 91, 182). Kohut admits that the Ukrainian gentry had to be reassured that their wealth and status would be recognized and that they would have access to positions in the central bureaucracy, but, "until Catherine took some measures along this line" (p. 188), they remained the most vocal defenders of the Hetmanate's political separateness. In other words, the Ukrainian gentry were ready to sell their "rights and privileges" in return for Russian recognition of the enserfment of the peasantry and most of the Cossacks. This, too, must have caused Khmel'nyts'kyi to turn in his grave.

It is not that Kohut is unaware of these issues. He is

too good a historian for that. It is that he writes history in the manner of Isabel de Madariaga, in which the mere description of documents is supposed to recreate history in its unadulterated beauty. Had he been willing to look more critically at his sources, he would have seen that it was not an irresistible Russian centralism that destroyed the Hetmanate's autonomy but a convergence of imperial interests with the greed and ambition of the Ukrainian gentry. He would have also avoided the central contradiction of his book: on the one hand, that the "rights and privileges" of the Ukrainian nobles were destroyed by Russian centralism and, on the other, that the Hetmanate's nobility became aware of the many benefits that would accrue from the new order only after the annexation.

This criticism is not intended to detract from the value of Kohut's book. The issue of Russia's relations with its borderlands, especially with the Ukraine, is a politically sensitive one, and there is much room for honest disagreement. Kohut has provided us with an authoritative introduction to a very complex subject. This study will serve as a base and a challenge to future historians to continue the exploration of this fascinating topic.

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JOANNA HUBBS. *Mother Russia: The Feminine Myth in Russian Culture*. Bloomington: Indiana University Press. 1988. Pp. xvii, 302. \$29.95.

Joanna Hubbs has attempted to explore the origin and development of the myth of the feminine in Russian society, which, she argues, "is perhaps the central mystery of the Russian people" (p. 237). She examines the female aspects of popular religion, folklore, epic poetry, political theory, and literary classics. Extensive notes, bibliography, index, and thirty-two illustrations complete this beautifully published monograph.

Hubbs seems to have taken inspiration from the semiotic binary paradigm of Russian culture, coupled with feminist theories of prehistoric matriarchy. She divides the Russian universe into two opposing spheres. On one side is the "feminine," comprising women, family, paganism, earth, and peasantry arising out of the depths of the matriarchal Russian past. On the other side is the "masculine," the alien, oppressive, patriarchal order of the state, church, war, and ruling class. The exploitative "Varangian" monarchy conquered the woman-centered Slavs and forced their compliance with the male-dominated Christian religion. The women (and peasants) resisted by taking their worship underground and gradually altering symbols of church and state to reflect their own beliefs. Later, leading Russian writers—Alexander Pushkin, Fedor Dostoevsky, Leo Tolstoy, Maxim Gorky, and Alexander Solzhenitsyn—resisted the despotic state by reclaiming Russia's lost feminine tradition.

To prove her thesis, Hubbs draws on ethnographical

materials collected in the nineteenth century and the finds of Eurasian archeological expeditions, many presented in English for the first time. The limited number of sources is a major weakness. Although all but thirty pages of the book concern the period before the eighteenth century, the author uses only a scant handful of medieval texts, often misquoted and out of context, filtered through secondary works of uneven quality. Hubbs overlooks the most relevant primary sources and instead builds her reconstruction of medieval society on the slippery ground of ambiguous folklore and peasant ritual. She fails to demonstrate that the vestiges of motif and design reflected living beliefs in the medieval period; they certainly did not when they were recorded in the nineteenth and early twentieth centuries. Furthermore, her selected bibliography indicates that she neglected some of the best scholarship pertinent to her topics: Russell Zguta's monograph on the *skomorokhi* (male pagan leaders), Charles J. Halperin's articles on the political symbolism of the Russian land, V. O. Kliuchevskii's and L. A. Dmitriev's books on medieval saints' lives; Maureen Perrie's study of the popular image of Ivan the Terrible; Robert O. Crummey's book on the Old Believers; and N. L. Pushkareva's—or any modern scholar's—research on the status of women in medieval Russia. Had Hubbs consulted these sources, she might have wanted to alter her thesis.

The author lacks a secure background in medieval Russian history, as evidenced by factual errors. The Kievan ruling dynasty of the eleventh and twelfth centuries cannot be legitimately described as "Varangian" (if it ever could). Medieval Russia boasted many more than the five women saints whom Hubbs enumerates. St. Augustine's writings on women and marriage did not circulate in the Orthodox East. The law code *Russkaia pravda* dates to the eleventh century, the time of Iaroslav the Wise and his sons, not to the twelfth century and Vladimir Monomakh; the code does not reflect a matrilineal system. Genghis Khan did not lead the campaign against Russia; he died a decade earlier. Sylvester, the putative author of the didactic essay on housekeeping *Domostroi*, was not a monk but a married priest. Women were not "divested of all legal power" (p. 191) in the sixteenth century but continued to enjoy substantial property rights and juridical protection. Many of the author's linguistic arguments are similarly flawed. For example, the word *rod* (clan) referred to both the male and the female lineage, not only the female.

Hubbs presents a host of untenable assertions. The medieval Russian household, both peasant and aristocratic, was patriarchal; chronicle and archeological evidence demonstrates that primordial matriarchy had vanished long before the establishment of the Kievan state and the conversion to Christianity. Orthodox clergy ought not to be characterized uniformly as "missionaries," or believers as "converts." The male-dominated pagan pantheon was not invented by the Orthodox church to supplant female paganism. The

veneration of Mary was well established in Eastern Christianity long before the baptism of Russia; it was not developed by Russian clergy to coerce women into the faith or by Russian women to "paganize" a religion they abhorred (pp. 94–95). The few clichéd admonitions against evil women in the sources do not suffice to justify a contention that women united to form a "fifth column" (p. 165) opposing Christianity and the patriarchal state. Men also were condemned for non-Christian conduct, nearly as often as women. Men as well as women venerated the Mother of God and Saints Paraskeva, Nicholas, and Boris and Gleb; all were invoked as protectors of the Russian state and ruling class. Finally, Hubbs misunderstands the nature of *duoverie*, which she mistranslates as "two faiths" (pp. 13, 92). It means, correctly, "dual-faith"—the merging of pagan and Christian ideas and practices into a rich, multifaceted, distinctly Russian religious system.

Conceptual frameworks, archeology, and ethnography, when thoughtfully applied, can be valuable tools for historical analysis. But, by using them recklessly to the exclusion of documentary sources, Hubbs distorts more than she clarifies. Readers should proceed, if they must, with caution.

EVE LEVIN

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RUSLAN G. SKRYNNIKOV. *The Time of Troubles: Russia in Crisis, 1604–1618*. Edited and translated by HUGH F. GRAHAM. (The Russian Series, number 36.) Gulf Breeze, Fla.: Academic International Press. 1988. Pp. xv, 325. \$25.00.

Ruslan G. Skrynnikov's book promises a great deal. Its subtitle suggests that it is a general study of Muscovite politics and society between 1604 and 1613, an "original manuscript prepared by the author for first publication by Academic International Press." In his historiographical introduction, the author suggests that he intends to elucidate the "interrelationships between the social and national liberation movements" (p. xiii) of the time more thoroughly and concretely than his predecessors.

In light of these claims and aspirations, the book is disappointing for a number of reasons. First, the work is original only in a very limited sense. Large sections of it consist of a verbatim translation of the author's previous work, *Minin i Pozharskii* (1981), published in the well-known series of biographies for the general reader, *Zhizn' zamechatel'nykh liudei*. The translation of *Minin i Pozharskii* stripped of some of the more flamboyant expressions of Soviet patriotism is not, in itself, a bad thing. The author's decision not to revise his earlier work more thoroughly, however, has produced a strangely unbalanced book. He devotes two-thirds of it to an extremely detailed narrative of the years 1611–13 and deals quite cursorily with the rest of the period indicated in his subtitle.

Second, the book consists largely of chains of factual statements with virtually no interpretive framing or commentary. In his selection and ordering of these facts, Skrynnikov, like any author, reveals his own preferences and commitments. He writes in the spirit of modern Russian patriotism and populism. More than anything else, he sees the Time of Troubles as the struggle of the Russian nation as a whole to defend its independence and, simultaneously, as the effort of lesser nobles, Cossacks, townspeople, and peasants to establish their right to participate in deciding the fate of the community. Religion and the Orthodox church, in particular, play little roles in his understanding of the period. More specifically, Kuzma Minin and D. M. Pozharskii emerge as heroic figures and genuine leaders of the Russian nation. In contrast to them, most of the boyars and officials were corrupt, self-interested, and unpatriotic. In Skrynnikov's view, the Time of Troubles ended in tragedy precisely because this elite group regained its power and pushed aside the people and social forces that had just restored the nation's independence and dignity.

Stylistically, the author's decision to present the reader with a chronicle-like narrative creates two problems. In spite of the promises made in his title and preface, Skrynnikov gives the reader little sense of the broader meaning of the events he describes. To cite one example, he evidently views many of the movements of the time as genuine expressions of popular grievances and aspirations. But at no time does he explore systematically the social and economic structure of Muscovite Russia or the ways in which social tensions expressed themselves in the chaotic events of the period. Moreover, the author's narrative method makes the book tedious to read, an odd achievement given the dramatic nature of the events chronicled. I found myself longing for any general statements to illuminate the catalogue of specific factual statements and frustrated by the author's apparent reluctance to use any organizing principle other than chronological sequence to structure the paragraphs and chapters of his work.

Skrynnikov's work is by no means without merit. His detailed reconstruction of certain episodes, for example, the process of electing Mikhail Romanov to the throne, is fresh and interesting. Moreover, he strives to make the early seventeenth century come alive for his readers by weaving colorful details drawn from contemporary sources into his narrative. He also makes a valiant attempt to explain the attitudes and actions of the men and women of the time in seventeenth-century, not twentieth-century, terms. Doing so, of course, involves guesswork that appears forced and arbitrary on occasion.

The ultimate purpose of this edition is to present a new synthesis of the Time of Troubles to the English-speaking reader. In some ways it works well. By and large, Hugh Graham's translation is accurate and clear. His decision to render the tricky terms *zemskii* and *zemskie liudi* as "national" and "nationalist," however,

introduces a somewhat anachronistic note into a discussion of confusing seventeenth-century realities.

In the end, however, the book's conspicuous lack of narrative shape and interpretive scheme defeats the editors' purpose. The English-speaking reader who seeks a brief and readable account of the period is still much better served by John T. Alexander's translation of S. F. Platonov's *The Time of Troubles* (1970).

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R. G. SKRYNNIKOV. *Samozvantsy v Rossii v nachale XVII veka: Grigorii Otrep'ev* [Political Impostors in Russia at the Beginning of the Seventeenth Century: Grigorii Otrep'ev]. (Stranitsy istorii nashei rodiny.) Novosibirsk, USSR: Nauka, Sibirskoe otdelenie. 1987. Pp. 218. 75 k.

Tsar Dmitrii Ivanovich, Russia's most famous pretender, has been neglected in Soviet historiography. The appearance of a new biography by the Soviet Union's leading specialist on the period, R. G. Skrynnikov, is therefore welcome. Dmitrii's short reign occurred during Muscovy's Time of Troubles, a tragic period following the extinction of the ancient ruling dynasty and the rise to power of Boris Godunov. In 1604 Muscovy was invaded by a small army led by a man claiming to be Dmitrii, the youngest son of Ivan the Terrible. The pretender declared that he intended to depose the "usurper" Boris Godunov. Tsar Boris's army stopped Dmitrii's advance, but when Boris suddenly died, in April 1605, a mutiny in the tsar's army and a rebellion in Moscow brought the pretender to power. Since it is generally agreed that the real Dmitrii had died as a child in 1591, Skrynnikov begins the biography with an investigation of who the pretender really was. He plausibly identifies him as a runaway monk named Grigorii Otrep'ev but offers no new evidence on the question. Skrynnikov tells the story of the young Otrep'ev very well, including his connection to the Romanov clan and his flight to Poland-Lithuania. The author's assertion that King Sigismund III was deeply involved in the pretender scheme, however, is based on shaky evidence.

The biography is strongest when dealing with Otrep'ev's campaign for the Muscovite throne. Skrynnikov dispenses with the usual Marxist rhetoric about Dmitrii coming to the throne because of a "peasant war." He demonstrates that Otrep'ev's civil war was not a social revolution and that, once in power, the new tsar was actually more conservative in his social policies than Boris Godunov. Skrynnikov also wisely rejects the opinion of V. I. Koretskii that Tsar Dmitrii faced a growing revolt of the masses and contemplated the abolition of serfdom. The biography unfortunately deteriorates when Skrynnikov describes the character of Otrep'ev. Ignoring contradictory evidence, the author accepts uncritically virtually every negative comment about Dmitrii found in the sources. He views him

as a cruel, treacherous, and lecherous person who masqueraded as a gentle monarch. It is an unconvincing portrait. Skrynnikov inadvertently chronicles the reasons why Dmitrii was regarded by many as a "good tsar." He granted pardons to his rivals, appeared as the defender of his people, and lowered the tax burden of many Russians. The weakest part of the biography concerns Otrep'ev's relationship to his royal council. Citing only evidence that supports his view, Skrynnikov declares that from the beginning of his reign Dmitrii was a virtual captive of the boyars. They effectively ran the state and prevented the tsar from carrying out any independent policies. Skrynnikov's image of a weak ruler shaking in fear before widespread aristocratic opposition is unconvincing. In fact, Otrep'ev's complete lack of concern about plots against him seriously undermines Skrynnikov's interpretation. It is doubtful, as the author asserts, that a desperate Otrep'ev intended to invite the Cossack adventurer "Tsarevich Petr" to Moscow to punish the boyars, and it is simply absurd to claim that the tsar contemplated fleeing Muscovy.

Despite these problems, Skrynnikov provides fascinating details concerning Otrep'ev's assassination, which he identifies as an exceptionally important landmark in the history of the Time of Troubles. It provoked a powerful civil war that nearly destroyed Muscovy. Skrynnikov's biography of Grigorii Otrep'ev partially fills a gap in Soviet historical literature, but this fascinating individual is worthy of a more careful study.

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V. R. TARLOVSKAIA. *Torgovlia Rossii perioda pozdnego feodalizma: Torgovye krest'iane vo vtoroi polovine XVII-nachale XVIII v.* [Russian Commerce in the Period of Late Feudalism: Trading Peasants in the Second Half of the Seventeenth-Beginning of the Eighteenth Century]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1988. Pp. 159. 1 r. 80 k.

This is an important and original study of Russia's internal economy during a crucial period of political and economic transformation. V. R. Tarlovskaiia examines the formation of a national market and the peasants' contributions to its creation. She concentrates on six districts between the Volga and Oka rivers, a region known as the "Nizhnii Novgorod Mid-Volga." She presents a picture of a domestic economy steadily becoming more expansive and complex. In these processes, trading peasants played a central role. Tarlovskaiia's study provides a much fuller description of the dynamics of internal trade than we have had before.

After a lengthy historiographical introduction that is balanced and fair, Tarlovskaiia organizes her work around four main topics. These include a geographical description of the trading villages and their population, the ambiguous political treatment of the trading peas-

ants, the organization of peasant trade, and the methods of accumulating capital. Tarlovskaiia shows that the trading peasants were not a uniform social group as they are often described but were composed of diverse kinds of people, whose motives and actual engagement in trade widely differed. In the villages she examines, Tarlovskaiia found that only five to six percent of the population actually tried to reach beyond the limits of their villages to a wider market. But she rightly argues that the peasants whose vision and aspirations did extend beyond the village had an economic influence far beyond their small numbers. Although not legally belonging to the townspeople, the trading peasants fulfilled in reality many of the economic functions that the merchants were intended to perform in theory.

In her conceptual approach Tarlovskaiia follows Soviet scholars I. D. Koval'chenko and L. V. Milov, whose innovative works have traced the expansion of a trading network linking village and town. Like her mentors, she has written a study close to the archival sources. She draws heavily from local materials, including the documents of provincial chancelleries, the census records, and especially the customs books on internal trade, in order to paint a meticulous picture of economic life.

The third and lengthiest section of her book treats the organization of peasant trade. Here, Tarlovskaiia makes her most important and original contribution. She brings out the social context of economic behavior, an attempt that is rarely made in many other studies. Especially helpful are her treatment of the Volga River as a stimulus to peasant trade; her discussion of the church peasants of the Blagoveshchenskii monastery; her emphasis on the significance to peasant trade of government contracts issued to provision the army and to feed the new city of St. Petersburg; and her description of individual peasant families, most of whom are depicted here for the first time. Her examination changes the prevailing picture of the internal economy. Traditional and often noneconomic elements shaped the framework in which commercial operations took place. What emerges from her account is the importance, in the actual conduct of trade, of such societal features as family relationships, physical violence, and communal behavior, as well as the constant fear of financial ruin.

Tarlovskaiia's study has several shortcomings. In a work so dependent on geography, the inclusion of a detailed map would seem imperative. Moreover, relationships between nobles and serfs were more complex than, as she describes them, having been purely economic affiliations. The customs books on which Tarlovskaiia relies have major gaps, although she admits their deficiencies and supplements them with other statistical sources.

Tarlovskaiia has provided a carefully researched study of a multifaceted topic in Russian economy and society. It is hoped that her book, in its approach and

content, will stimulate similar works on other regions of Russia.

WALLACE DANIEL
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M. M. SAFONOV. *Problema reform v pravitel'stvennoi politike Rossii na rubezhe XVIII i XIX vv.* [The Problem of Reform in Russian Governmental Policy at the Turn of the Eighteenth and Nineteenth Century]. Leningrad: Nauka. 1988. Pp. 247. 2 r. 60 k.

M. M. Safonov provides a detailed account of reform efforts made by high officials in St. Petersburg from March 1801 to March 1803, from the murder of Tsar Paul I and accession of Alexander I to the "liquidation" of the senate's right of "representation." The author's goal is to "explain the specific reasons that tsarism undertook reform" and "to provide the most detailed, factual history of the consideration of reform and of the struggles that resulted" (p. 26). The introductory chapter on the historiography of the subject points out that Alexander I's reforms were taken up first in the 1860s by scholars who were interested in the Great Reforms of that time and who "made no effort to hide their contemporary interests" (p. 2). The first, M. I. Bogdanovich, focused his study on the personality of Tsar Alexander I, who hated despotism and sought not particular reforms but a fundamental reconstruction (*korennaia perestroika*) of the state order. Safonov makes no reference to his own contemporary interests and does not use the word *perestroika* again, but he obviously thinks it worthwhile to explain clearly just what frustrated the efforts of able, well-motivated reformers in the early nineteenth century.

A particular strength of the book is the detail. The author clearly has mastered the sources, both printed and archival, and the Russian language secondary literature as well, from Bogdanovich's 1866 article to N. V. Minaeva's 1982 Saratov dissertation. Although he pays no attention to Marc Raeff or any of the other Western scholars who have made significant contributions to understanding the nature of reform in the Russia of Alexander I, Safonov's work is itself a worthwhile contribution, providing a good deal of new information and well-informed reconsideration of the well known. Safonov's earlier published works, descriptions and analyses of particular documents such as the peasant "project" of P. A. Zubov, have appeared in serials such as *Sovetskie arkhivy* and brought to light much new information. The second strength of the book is the clear presentation of these documentary materials in the context of a broad and well-informed discussion of the issue of reform.

Safonov finds that the reformers understood the need for reform to overcome the "obstacles" that stifled the growth of productivity in social and economic terms and to develop institutions capable of mobilizing Russia's resources to meet many complicated problems. In particular, the tsar and his "young friends" repeat-

edly demonstrated both understanding and energy in working toward reform. The tsar well knew that he had to end serfdom and the nobles' monopoly on privilege, and, indeed, to end autocracy itself. Nonetheless, from serious work on serfdom and on making the senate an important center of political power, Alexander soon turned, if not retreated, to the more manageable task of reforming the central government's executive agencies. The reasons for that turn in the reformers' interests included not only the opposition of beneficiaries of the status quo but also the difficulties and complexities of the issues and the inability, or at least unwillingness, of the reformers themselves to compromise. Alexander's young friends, for example, repeatedly buried reform proposals, such as Zubov's proposal on serfdom, that might have proved beneficial steps, because the proposals came from individuals or groups other than their own whom they mistrusted or resented for one or another reason.

Safonov concludes that the "wide ranging consideration of a program of social-economic and political reform" (p. 243) in the end produced only a restructuring of the state apparatus. That facilitated centralization and strengthened autocracy but also showed that the government was incapable of solving the country's main problems. The implementation of "cut-back reforms" did not end consideration of the issues. Within a decade they were raised in the plans of M. M. Speranskii and were soon raised again not in reform plans but in the "revolutionary, Decembrist way" (p. 243).

JAMES T. FLYNN
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SCOTT J. SEREGNY. *Russian Teachers and Peasant Revolution: The Politics of Education in 1905.* (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 1989. Pp. x, 292. \$27.50.

The revolution of 1905 surprised contemporaries and continues to intrigue historians by the extent to which previously unorganized or entirely inert groups suddenly showed a capacity for collective mobilization and even for disciplined political action. Normally either violent or passive, peasants signed petitions and formed political organizations. Usually timid and culturally isolated, rural schoolteachers joined forces both with representatives of the peasant community and with zemstvo liberals in pressing for social change and political reform. It is the process of social interaction in the countryside, which propelled the normally reticent teachers into the political fray, that constitutes the subject of Scott J. Seregny's book.

This focus allows Seregny to address some of the central issues of 1905. To what extent did peasants take part in coherent political movements? What kind of influence did the rural intelligentsia exert on the people's behavior and beliefs? How authentically popular

were the organizations formed in the peasantry's name? How genuinely representative were the organizations of the teachers themselves? His story challenges a number of stereotypes, both contemporary and retrospective. The authorities viewed the peasants as dangerously volatile and the third zemstvo element as the instigators of popular unrest. They refused to recognize peasant organizations as true expressions of popular interests. Recent historiography (in particular the work of Ben Eklof) has insisted, by contrast, on the vast social chasm that separated the teachers from village inhabitants and prevented them from exercising any significant cultural influence, let alone a political one. At the same time, Eklof also emphasizes the self-consciousness and rationality of peasant behavior.

Agreeing that peasants often acted with clever calculation in pursuit of collective goals, Seregny denies that they kept their distance from other groups. Rather, he shows how an active minority of relatively sophisticated villagers and of mobilized teachers cooperated in the formation of a rural political movement. Searching for a mediating element to counteract the possibility of popular unrest, zemstvo liberals encouraged teachers to form organizations that they hoped would promote their own moderate political principles. At the same time, the peasants themselves appealed to teachers to act as guides and informants, interpreting the information that reached the villages and transmitting peasant demands to the outside. Finally, the teachers came to realize that no improvement in their own personal and professional situations could occur without changes in the larger political order.

Seregny thus demonstrates that peasants were indeed capable of purposeful political initiative: selecting representatives, soliciting outside aid, producing their own leaders, and adapting political slogans for their own use. He also insists that educated outsiders managed to overcome peasant distrust and helped shape peasant politics. Teachers supported the peasantry's radical social claims (for example on the land question) and propagandized for radical political goals (democratic reforms). Contrary to the official perception, however, they did not encourage violence but promoted peaceful, planned activity. By restraining the villagers' more extreme antigentry impulses, teachers in fact served the zemstvo liberals' purposes as the Social Democrats feared they might. But, as the Social Democrats were perhaps unwilling to acknowledge, their role also helped cement the powerful alliance of disparate social groups that accounted for the revolution's political successes.

After 1905 government repression reduced the rural teaching corps to the same state of inertia that it had experienced before 1905. In typically self-defeating fashion, Seregny suggests, the regime had destroyed a successful mediating force between educated society and the popular masses, thus contributing to the triumph of extreme political methods in 1917.

Seregny's concentration on the changing social relations generated by the revolutionary situation shows

how conflict and crisis spanned seemingly unbridgeable cultural gaps (between peasant and teacher, for example) and elicited hidden capacities for civic initiative and for creative adaptation to new institutional opportunities. The rural social map emerges from his account as a place of multidirectional influences and responses. Seregny bases this detailed story primarily on the reports of associational gatherings on the national and provincial levels. To these sources he adds insights gleaned from memoirs and archives. Despite the welcome dynamism of the book's underlying conception, however, the actors in this intelligently reconstructed tale remain largely faceless, and the account unfortunately lacks the excitement one might expect from a story of revolutionary conflict and political transformation.

LAURA ENGELSTEIN
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HANNU IMMONEN. *The Agrarian Program of the Russian Socialist Revolutionary Party, 1900–1914*. (Studia Historica, number 26). Helsinki: Finnish Historical Society. 1988. Pp. 169.

In this brief treatment of the Socialist Revolutionary party's (PSR) agrarian program, Hannu Immonen seeks to resolve "certain open questions" and "conflicting interpretations" found in earlier scholarship. He is interested above all in who conceived the party's agrarian program, how the nature of this program should be assessed, and to what extent the PSR successfully moved from abstract ideas to the development of plans for implementing its programs.

According to Immonen, previous studies wrongly attribute the PSR's agrarian program exclusively to Viktor Chernov, ignoring important contributions made by Panteleimon Vkhliav (who became deputy minister of agriculture in 1917) and N. I. Rakitnikov. In fact, he argues, Chernov's productive part in elaborating the party program actually ended in 1904 with the publication of a draft program. From 1905 to 1908, Vkhliav, Rakitnikov, and other agrarian specialists emerged as the most innovative thinkers on the question of land redistribution and in the process changed the emphasis of the PSR agrarian program "from socialism to egalitarianism" (p. 159). In 1905, partly in response to the Constitutional Democratic party's own radical agrarian program, Vkhliav adopted the "right to land" concept (*pravo na zemliu*) as a cornerstone of the PSR's program. Similarly, in the wake of the 1906–07 land reform under Petr A. Stolypin, it was Rakitnikov rather than Chernov who sought to adjust the agrarian program to new social realities by proposing to replace the commune with a system of small landholding as the basis of the PSR's land socialization policy. By the time the PSR drew up its 1907 proposal for a national land law based on the legal views of the peasantry, Immonen maintains that the party's agrarian theory had "passed out of Chernov's hands" and

into those of neopopulist agronomists like Vikhliaev. Vikhliaev and Rakitnikov thus played a much larger role in formulating party policy after 1905 than previously recognized, and their ideas proved especially significant in shaping the land law proposal of the 104 in the Second Duma.

Immonen contends that the proposed agrarian land law was a more important indicator of PSR political views than the agrarian program adopted in 1906. From an economic demand stressing noncapitalist evolution in agriculture, the PSR moved to a juridical demand for the creation of a new legal system that would regulate the right to land use and guarantee such a right to all. The proposal thus constituted a concrete scheme for implementing PSR demands to socialize the land, thereby laying to rest the argument that it was precisely the lack of such a plan that accounted for the party's failure in 1917. Finally, Immonen argues, the proposed land law "was a decisive step [toward PSR] acceptance of independent small landholding" and the idea that the land should belong to everyone. The party leadership's views on agrarian policy, therefore, were moving away from socialism toward egalitarianism "to be achieved through . . . enactment of a land law" (p. 159).

With only 159 pages of text, Immonen's book is, unfortunately, far too brief and narrowly focused to treat adequately even the limited set of questions raised at the outset. As a result, his conclusions appear more as suggestions because of lack of detail and depth. Coverage of the land law proposal and the entire period from 1906 to 1914, for instance, is limited to a mere forty-three pages. The important distinction between socialism and social egalitarianism remains unclear as do Chernov's activities and ideas from 1904 to 1917 (although the author does admit that in 1911 Chernov was forced to intervene in order to settle disputes arising from Rakitnikov's proposals). I am also not convinced that the scholars Vladimir Ginev, Manfred Hildermeier, Maureen Perrie, and Oliver Radkey (or authors of recent dissertations not treated in this work) ignore so completely the issues under consideration. In sum, although several of Immonen's suggestions are clearly intriguing, a more substantial study is still needed if one is to offer a revisionist account of the Socialist Revolutionary party or its programs.

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O. N. ZNAMENSKII. *Intelligentsiia nakanune Velikogo Oktia-bria, fevral'-oktiabr' 1917 g.* [The Intelligentsia on the Eve of the Great October Revolution, February to October 1917]. Leningrad: Nauka. 1988. Pp. 349. 1 r. 60 k.

O. N. Znamenskii begins this book by asserting that he will study the attitudes and behavior of the "bourgeois," "petty-bourgeois," and "village" intelligentsia in 1917. What follows is not a study but an indictment.

According to Znamenskii, the intelligentsia was disorganized, irresponsible, apolitical at best, and monarchist at worst. Its members were divided by differences in socioeconomic status and by the intelligentsia's chronic "individualism." They also lacked the contact with the working class that would have provided them with much-needed guidance. Lost in the "whirlwind" of the revolution, they drifted among parties and opinions. Most of them did not actually harm the development of the revolution but only because they avoided political organizations. After the Bolsheviks seized power, the party and the proletariat took on the long-overdue task of educating the intelligentsia about its proper relationship to society.

Such an argument amidst the rethinking of Soviet history unleashed by *glasnost'* seems obstinately antiquated, advanced as it was in 1988. Indeed, Znamenskii, who finds it difficult to let a paragraph pass without quoting Lenin, manages to end his book with a quotation from Gorbachev. This is the only indication that he is aware of the new revolution gripping his country. Unabashedly he states in his introduction that his main source on the history of the intelligentsia is Lenin's *Collected Works*. He also uses archival, periodical, and documentary materials as well as memoirs.

The book's narrative consists of descriptions of the intelligentsia's reactions to the revolution or, rather, of the reactions of selected individuals combined with broad, weakly substantiated generalizations about the rest of educated society. Znamenskii also discusses the formation of various professional organizations, concentrating on teachers and performing artists. This is hardly an exhaustive summary of intelligentsia activity in 1917. Nor has Znamenskii done any systematic study of the topics he has selected. Systematic study is not his purpose. Writing with the authority of a prosecuting attorney, he only advances his scraps of evidence to bolster the hoary accusation that intellectuals (save those who joined the Bolsheviks) dithered while the Bolsheviks led the proletariat to victory.

This kind of book used to be the norm for Soviet historiography. In the last several years, however, refreshingly original works based on solid, even innovative, research have appeared. The publication of books such as Znamenskii's indicates the continuing presence of conservatives within the ranks of professional historians. It is hoped that their time is passing and that this book is more relic than omen.

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FRANCESCO BENVENUTI. *The Bolsheviks and the Red Army, 1918-1922*. Translated by CHRISTOPHER WOODALL. (Soviet and East European Studies.) New York: Cambridge University Press. 1988. Pp. viii, 264. \$44.50.

Francesco Benvenuti's title is slightly misleading; what he really offers is a study of the army as a political issue within the Bolshevik party. He focuses on the opposi-

tion that challenged the Soviet regime's military policy at the eighth party congress in March 1919. He ties that episode to earlier Left Communist criticism of the creation of a regular army in 1918 and to subsequent controversies over the structure of the party organization in the army, military doctrine, and the goals of the army's educational efforts. Benvenuti has nothing new to say about any of these issues, and his sources are for the most part familiar. He tells the story in more detail, however, and connects the separate episodes together more systematically than others have.

Benvenuti argues that the Bolshevik military opposition was more important than we have believed. It was outvoted in 1919, but it was strong enough to win concessions, and it voiced concerns shared by many of the party leaders. According to Benvenuti, the military opposition forced the augmentation of the commissars' right to supervise military operations and an expansion of the role of Communist party organizations in the army, in both cases overcoming Trotsky's defense of functional specialization and hierarchical subordination. The Central Committee in the middle of 1919 compelled Trotsky to accept party policy on those matters that he had ignored, overruled him on military strategy, and arrested his commander in chief, Ioakim Vatsetis. The military opposition could not reverse the overall direction of military policy—and after 1919 no longer really wanted to—but it appealed to a variety of powerful impulses within the Communist party: suspicion of regular armies, preference for class-based institutions (impossible once conscription flooded the Red Army with peasants), distrust of regular officers who had served in the tsarist army, resistance to abridgment of the rights of local Communist party organizations, and belief both in egalitarianism among party members and in the special prerogatives of party members vis-à-vis outsiders. These party members were ideologically heterogeneous, but, as Benvenuti points out, even the most democratic among them were willing to settle for strong commissars as the guarantor of party and class interests in the army. They failed to see that the institution of commissar was inherently authoritarian. Benvenuti asserts, as others have, that the creation of the regular professional army and the militarization of the party organizations in the army were paradigmatic of the transformation of Soviet institutions during the Civil War.

Certainly Benvenuti is right to link the Left Communists, the opponents of military policy, the controversy over party structure in the army, and other episodes together, but the links are not necessarily those that he describes. Despite his thoughtful analysis, the deliberately narrow angle of vision that he has selected removes the story of military politics from much of the context needed to understand it. For instance, he points out that the controversies over the party structure in 1919 and economic administration in 1920 interacted with military questions, but in fact almost every other political conflict during the Civil War could find a place in the narrative. More troubling is that

Benvenuti does not consider (even to reject) the possibility that the personal and political rivalry among Trotsky and Stalin and Zinoviev may have been an independent factor bearing on the specifically military questions. He discusses the conflict between Trotsky and Stalin almost entirely in terms of the military issues that were in dispute; one would not know from his book that rivalry with Zinoviev played a part in most of the controversies involving Trotsky, which is to say most of those that Benvenuti treats. He also has little to say about how the creation of a regular army actually affected the local Communist military chieftains who were important during the early phase of the Civil War and the Red commanders who gradually emerged as a new professional elite. He asserts that their resentments were a key component of the opposition to military policy, but he provides no details that would help us to understand what was involved.

A few terminological glitches suggest (possibly unfairly, because they may be a result of the translation) incomplete command of the material. It probably does not much matter that the slogan of official nationality comes out "orthodoxy, autocracy, and patriotism" (p. 206). But one pauses when party leaders are sometimes said to argue over policy toward the "average peasant," at other times about "middle peasants." Repeated references to "members of the general staffs" (by which Benvenuti means general staff officers [the *genshtabisty*]) reveal ignorance of a key military group. Consistent use of the locution "debate in Congress," as though the party congress were a permanent legislature, is simply odd.

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DON K. ROWNEY. *Transition to Technocracy: The Structural Origins of the Soviet Administrative State*. (Studies in Soviet History and Society.) Ithaca: Cornell University Press. 1989. Pp. xv, 236. \$31.95.

Don K. Rowney's work has the ambitious goal of placing the political history of the USSR in a new perspective through a quantitative study of the Russian and Soviet civil service. Rowney argues that the 1920s witnessed the emergence of a powerful technocratic administration in the Soviet Union. This rising technocracy reduced many of the debates of party leaders, including Leon Trotsky, Nikolai Bukharin, and even Joseph Stalin, to near irrelevance and set the basic pattern for the development of the Soviet state.

Although Rowney has a good deal to say about government organization and structure, the core of his study is a statistical analysis of three generations of civil servants by age, education, and social background. The first of these generations, the "prerevolutionary," is something of a hybrid. Rowney charts the impressive advance of specialization and expertise among tsarist officials during the nineteenth century, but he maintains that a decisive characteristic of the pre-1917

government was the continued prominence of the nobility. In Rowney's view, nobles adapted to declining landholding by strengthening their participation atop central and, especially, provincial administration. Indeed, aristocratic "generalists" effectively immobilized the rising strata of experts. In so doing, they guaranteed that the peasant revolution in 1917 would sweep away the leading cohorts of tsarist administration.

The cataclysm of 1917 created two successor generations of state servitors. The first was a transitional group, drawn in significant part from the frustrated "sub-elite" of the tsarist era and clustered in the new, functionally specialized, and centralized commissariats. More important was the second rising generation of the 1920s. Recruited to a degree unprecedented in European history from the lower classes, this cohort was characterized by its relatively low levels of education and large resentment of the politically suspect transitional group. This last generation's need for training and simultaneous demand for social advancement, in Rowney's view, set the agenda for the 1930s. Taken together with the Soviet state's inheritance of centralized administrative structures, the third generation's rise to predominance constrained the regime's leadership and doomed radical policies, such as the effort to proletarianize the Communist party.

The book is written clearly and, at times, subtly. Rowney's statistical analyses will be accessible to almost all. Although painted with a very broad brush, his picture of administrative evolution and revolution shows important nuances. The portrait of rural administrators in both the pre- and postrevolutionary eras, for example, is detailed and instructive.

Still, the main contours of change charted here are open to multiple interpretations. Rowney's statistical analysis does not prove his contention that noble administrative prominence generally failed to hinder the autocratic state. Reformist bureaucrats such as Petr Stolypin or Sergei Witte would argue otherwise. More importantly, the generation of the 1920s does not resemble a united technocracy. The dreadfully low level of education and experience indicates a step backward rather than forward to technocratic specialization. The great social distance between white-collar Red experts in Moscow's commissariats and the unskilled worker-peasant Communists who flooded into the rural soviet apparatus raises doubts about generational coherence and potential for collective action. Rowney's chief example of their group assertiveness, the movement of worker Communists from the bench into white-collar posts, was not uniformly opposed by party leaders. What emerges from the study is a picture of a mixed and unsettled new generation who pressed the politicians in important ways but also were subject to direction and manipulation.

In sum, Rowney does not banish traditional political analysis from the field. Yet his work forces that analysis

deeper and indicates new dimensions that must be added to our understanding of early Soviet history.

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NORA LEVIN. *The Jews in the Soviet Union since 1917: Paradox of Survival*. In two volumes. New York: New York University Press. Pp. xxxiv, 525; 528–1013.

Often we speak of Adolf Hitler as having "put an end" to the history of European Jewry, because he managed to wipe out the Jews of interwar Poland and those of the West. Yet a glance at the map will tell that at least half of the Jews in the old tsarist "Pale of Settlement" ended up in the Soviet Union between the wars—some 3 million people by a count in the 1930s—and the Holocaust affected these only peripherally. There were still over 2 million people listed as Jews in the Soviet count of 1959, and, despite high emigration in the 1970s and large-scale evasion of the censors, there were 1.8 million in 1979.

The late Nora Levin's massive study describes, almost for the first time (Benjamin Pinkus has coincidentally published a considerably shorter study, *The Jews of the Soviet Union: History of a National Minority* [1988]), the entire separate history of this segment of European Jewry. She tells the story in thirty-four chapters (one suspects a lecture course in the background), but informally she lets it divide into three episodes. First there is the terrifying drama of the revolutionary years, during which this considerable body of Ashkenazim was abruptly cut off from the rest and simply ripped to pieces. These Jews were murdered not only in large numbers but also in the name of emancipation; their religious customs (including the Hebrew language), their history, and above all their traditional modes of economic livelihood were all alike (and simultaneously) abolished. Levin is here, of course, indebted to Zvi Gitelman's classic work on the *Evsheksii* (*Jewish Nationality and Soviet Politics* [1972]), but she effectively shifts attention away from the assailants to the Jews themselves, giving a more balanced impression than he did of what happened.

Levin's second episodic section regards the large-scale postrevolutionary reorientation of Jewish lives. Very suddenly in the late 1920s, for example, almost ten percent of the Soviet Jewish population, seeking a new occupational mode, moved out of the *shtetlach* onto the land and became farmers. In the early 1930s collectivization put an end to that particular sort of reorientation, but, meanwhile, in an equally dramatic surge of population, Jews moved into the cities of the Russian republic and into the Soviet school and university system. Step by step during the Stalin revolution huge numbers of Jewish tradespeople and artisans transformed themselves in these new locations into members of the "worker-peasant" middle class. Thereupon the war finished off the helpless remains of the Old Pale Jewry but at the same time cemented the

integration of a huge, largely modernized body of Jews into Soviet society. The story of this, the least explored part of Soviet Jewry's history, is the most lucid and original part of Levin's book.

The third great phenomenon recounted in this book is the rebirth of consciousness that has taken place since 1953, a rebirth precipitated by the outright persecutions of late Stalinism and then crystallized during the long "stagnant" Brezhnev years, in part by a rediscovery of the past, in part by the opportunity to emigrate. Here Levin is paralleling the recent work of Benjamin Pinkus (*The Soviet Government and the Jews* [1984]) and others. This part is less detailed, but she makes a greater effort to construe what Jews can do in the Soviet Union, as well as what is forbidden, and in this sense her book strikes me again as well balanced.

Levin claims in her preface to address "nonspecialists" and "undergraduate students" above all, but she actually writes on an exceedingly sophisticated level, and, in chapter after chapter, it is not just secondary literature but Yiddish language primary source material that underlies her tale. She provides almost two hundred pages of footnotes, two dozen statistical tables, several maps, dozens of photographs, a five-page chronology, and a useful glossary. No specialist can afford to miss this important book. One may nonetheless note some peculiarities that detract from the overall effect. One regards the absence of Russian language references. Everyone knows that a good deal of the material in Soviet journals cannot be taken seriously as hard evidence and that in recent years an enormous amount of material issuing from *samizdat* has been translated into English. Yet the other major books about Soviet Jewry refer to Russian material with some frequency, and since Levin (judging from a few references in the notes) did seem to be a master of the language, it is unfortunate that she published so essential a book without making the extra effort to use the Russian materials. She did not even give the Russian references for her statistical tables but lifted them from secondary sources.

Levin is correspondingly neglectful of the major secondary works about Soviet history. It is difficult to pinpoint this failing, because the book lacks a general bibliography and because important references show up unexpectedly. Typically, while discussing the late Stalin period, she relies heavily on Franz Borkenau's essays published in *Commentary*, which is wonderful because he was an astute observer who is today often forgotten. But she makes no mention of Boris Nicolaevsky, who published in the *New Leader* and the *Sotsialisticheskii Vestnik*, although he was also very astute (and thoroughly interested in Jewish affairs). In chapter 25, when discussing Nikita Khrushchev, Levin refers to Edward Crankshaw, Roger Pethybridge, and one of Carl Linden's articles, but not to Linden's book or the work of Myron Rush, John Armstrong, Zbigniew Brzezinski, or Boris Meissner, although these scholars have written the major books on the period. Her

textual account of Khrushchev's rise and of 1956 is as uneven as one might expect with such omissions.

No scholar can do everything, and Levin's book is so interesting that one is willing to forgive the author a great deal. But one does wonder in this connection about the rather jagged introductory remark: "... I find no rapport or much sympathy with certain of the modern strained efforts to find 'inner structures' in the history of human communities, or quantitative techniques" (p. xix). Since some of the book's diagrams and statistical charts are themselves imperfect (for example, the boundaries in maps 7.1, p. 156 and 15.1, p. 336; and the numbers in table 26.1, p. 587), one wonders how she could afford such snobbery about the modern social history that she herself was writing.

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NEAR EAST

ÇAĞLAR KEYDER. *State and Class in Turkey: A Study in Capitalist Development*. New York: Verso. 1987. Pp. 252. \$39.95.

Çağlar Keyder sees class formation in Turkey as a stratification process that is bound to follow the Marxist blueprint, even though the evidence cited often in this book contradicts that assumption. Keyder attempts to prove that the bourgeoisie began to emerge fully in the 1960s and some ten years later acquired political power, mainly through "import substituting industrialization" (which in plain language means manufactured goods produced domestically to replace the imported ones) and a political liberalism promoted under U.S. hegemony. All of these assumptions coincide neatly with the theory of peripheralization and dependency—gleaned largely from radical Latin American literature—which Keyder espoused in a Ph.D. dissertation (published later as a book, *The Definition of a Peripheral Economy, Turkey, 1923–1929* [1981]). There is no quantitative data provided to support the sociopolitical hypothesis and to demonstrate how the social stratification in the Ottoman and republican periods linked to each other. It is true that the author deals in the first three chapters with the Ottoman background of class formations, but he somehow arrives at the wrong conclusion that by 1923 Turkey did not have a "bourgeoisie," because his bourgeois, which "could to a large extent be identified with non-Moslem merchants" (p. 78), had left the country in the period between 1918 and 1922. In effect, therefore, Keyder's bourgeoisie consists almost exclusively of the merchant and manufacturing sectors. Presumably, in his view, there was not a significant agrarian bourgeoisie because the Ottoman state did not possess a class of large landholders, as the author rightly claims. The main adversary of the bourgeoisie, according to Keyder, was the bureaucracy, whose statism helped but then antagonized and hampered the emerging bourgeoisie, the most liberal elements of which were the small group of the *dönme*, or

converted Jews. These are ideological fantasies. Actually, the economic backbone of the middle classes, in both the Ottoman empire (second half of the nineteenth century) and the republic, consisted of owners of small land parcels, who were largely ignored by the press and the elites but continued to grow in size and wealth by performing crucial roles as communal leaders and merchants (buying and selling land produce) and, after 1945–46, assumed grass-roots organizational and mobilizing roles. (In the early 1960s the military, applauded by the modernist intelligentsia, closed the party precincts at the grass-roots level, allegedly in order to prevent dissension but actually to monopolize power in the hands of the upper-class elites, who were assumed to be attuned to the principles—read secularism—of the republic.) Keyder ignores these issues. If he were to regard social stratification as a complex process determined not only by economic factors but also by a myriad of cultural, historical, and sociopolitical forces and values, as a true sociologist would do, his approach and conclusions would be different. Like many other Turkish scholars infatuated with radical theories and anxious to prove an identification with modernity and progress, as well as detachment from “national” biases at all cost, Keyder fails to pay attention to the basic fact that class formation in Turkey, with its interest motives, vies with a sense of community, deep attachment to social hierarchies, and deference to elites and authority, which intermingle and often produce unexpected results.

The main shortcoming of the book, in addition to some factual errors and selective and incomplete use of sources, is its preordained scheme, which is only marginally related to the real sequence and the sociopolitical dynamics of transformation. In essence, the book is the history of Turkish politics in the republic already told by others in various and better ways. Yet it is full of tantalizing ideas, occasional incisive analysis, and useful information (notably chaps. 7 and 8 on economic development, where Keyder is at his best). This book reveals the author to be a man of capable, maturing intellect with good promise for the future but in too much of a hurry to build ingenious theories of social change and too alienated from his own society to understand its true culture and complex stratification patterns.

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IRVIN C. SCHICK and ERTUGRUL AHMET TONAK, editors.
Turkey in Transition: New Perspectives. Translated by
REZAN BENATAR et al. New York: Oxford University
Press. 1987. Pp. xii, 405. \$42.00.

The articles collected in this book constitute a valuable inquiry into the history and politics of Turkey since 1923. All of them are written by Turks who provide perspectives that are not readily available to the non-

Turkish reading public. All of the authors belong to the generation that matured in the 1960s in the liberal environment created by the revolution of 1960 and the constitution of 1961, which for the first time permitted open intellectual and political debate. During these years, Turkish intellectuals began to reexamine and reevaluate their country's past as well as its contemporary politics. This anthology is therefore largely the fruit of the revisionist school of Turkish historiography, a school hardly familiar to readers in the United States and the West.

The book is divided into three parts: “Background and Political Development”; “Political Forces”; and “Economic Development.” Part 1 opens with Taner Timur's essay “The Ottoman Heritage,” whose purpose is to challenge the official thesis that the Turkish republic marked a clean break with its Ottoman past. Taner emphasizes continuity and shows that the republic retained a great deal of the past. Although he admits that the new regime was revolutionary, he argues that, “by the 1930s, the revolution was officially over and even the word ‘revolution’ was no longer uttered” (p. 7). Here he exaggerates. As late as August 1938 the official party line placed great emphasis on “revolutionism,” noting that “in the administration of the affairs of State the Party does not hold itself bound by the principles of gradual progress and evolution. It is essential for the Party to remain faithful to the principles born of the revolution” (see the special supplement “The New Turkey” in *The Times* [London], August 9, 1938).

The policy “designed to consolidate and develop” the class structure, which Taner describes as being “devoid of revolutionary social content” (p. 7), was in fact revolutionary in so far as its aim was to put precapitalist Turkey on the path of capitalist development. This is Çağlar Keyder's thesis in his two essays, “The Political Economy of Turkish Democracy” and “Economic Development and Crisis: 1950–1980,” in part 3. This theme is amplified in his book *State and Class in Turkey: A Study in Capitalist Development* (1987). Within the parameters of the first two essays, which bring the reader into the 1970s, Stephane Yerasimos and Cem Eroglu analyze in detail the monoparty period (1923–45) and multiparty rule (1945–71) respectively. Both essays analyze developments in Turkey in terms of its political economy rather than modernization or Westernization. This approach provides a more complete picture and is a welcome change from the conventional approach.

The political forces discussed in part 2 are the Left, the ultranationalist Right, the religious Right, and the army. The author of the essay on the Left is Murat Belge, who was and is active in the Turkish Left and who was forced to write under the pseudonym Ahmet Samim during the period of military rule following the coup of September 12, 1980. His essay is an unabashed critique of the Turkish Left, which had been smashed once again by the army, and reflects the demoralization and frustration of a participant.

The other three essays are scholarly accounts of

three important topics that deserve more attention. Mehmet Ali Agaogullari's essay on ultranationalism is particularly welcome because very little has been written, especially with such rigor and command, on this important topic. After describing the birth of this movement and its development during the republic, the author presents a fascinating discussion of the neofascist National Movement (or Action) party (NMP). This party played a crucial role in the terrorism that destabilized Turkey in the 1970s and prepared the ground for military intervention. Agaogullari shows that the party in which retired officers played a prominent role infiltrated the army and tried to work hand in glove with it. When the party was dissolved in October 1981 and its leaders were on trial, they petitioned the court to acquit them because "the requests [aims] outlined in the NMP manifesto have been realized [by the military regime of 1980]. . . . The attitudes and actions of Alparslan Turkes and his party are today actually under implementation" (p. 206).

Binnaz Toprak's essay on the Islamic Right remains timely. She discusses the role of Islam in Turkish politics and society and concludes that "a mass political movement based on religious appeal has little chance of success in modern Turkey" (p. 230). Semih Vaner's essay on the army provides a provocative analysis of an institution that plays a critical role in contemporary Turkey, which has been marked by military intervention every decade since 1960. Unlike many scholars, Vaner does not see the army as a monolithic and unchanging institution; on the contrary, he finds it adaptable and dynamic, able to adjust to a rapidly changing society. Indeed, far from being monolithic, the army has factions or "parties" in constant competition. With this perspective, the reader has no difficulty in comprehending the changing character of the three coups—1960, 1971, and 1980—for each reflected the changing character of both the institution and the society in which they took place.

The four essays in part 3 on economic development provide a wealth of information and fill in gaps left in earlier chapters. Margulies and Ergin Yildizoglu discuss changes in agriculture, 1923–70, arguing that, although there was no great concentration of land in terms of ownership (as others claim), landlords dominated the peasantry through economic control (p. 286). Çağlar Keyder examines economic developments after 1950 in the context of a growing crisis, and Alpaslan Isikli's essay, "Wage Labor and Unionisation," reflects on the role of the working class in the political economy. This is an important and original essay, and it could easily have been placed in the section on political forces. Finally, the editors' essay, "The International Dimension: Trade, Aid, and Debt," and their conclusion round off an anthology that will remain an important contribution to our understanding of modern Turkey.

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MOSHE MA'OZ. *Asad: The Sphinx of Damascus; A Political Biography*. New York: Weidenfeld and Nicolson. 1988. Pp. xiv, 226. \$19.95.

Hafiz al-Asad is widely regarded as the most remarkable ruler of modern Syria, but there is no agreement on whether he is a brilliant statesman or a ruthless Machiavellian self-seeker. Moshe Ma'oz, an Israeli scholar, seeks to answer these questions about his country's archenemy. The author, drawing chiefly from the existing literature, reviews Asad's career, from the time that he joined the Ba'th party in 1947 until his displacement of all rivals in 1970, and then presents a lengthy firsthand investigation of Asad's rule.

Asad appears as a highly ambitious Machiavellian who is also motivated by sincere concern for his people and nation. He is devoted to the promotion of Greater Syrian and Arab unity and the defeat of Israel, but this action also serves to expand his Alawi, Ba'thist, and army power base and to legitimate his sectarian military regime. During his first "six good years" (chap. 7), he had some success in improving the economic condition of the populace and in replacing the old elite with a new one drawn from the deprived members of Syrian society. But Syria's chronic economic problems remained unsolved, and corruption in government and business grew. These defects were offset by success in preparing an Arab crusade against Israel. Devotion to Arab solidarity against Israel bore fruit in the war of 1973, but, as Arab solidarity dissolved, Asad turned first to Fertile Crescent unity and then to Greater Syria. He had some initial successes with Jordan, the Palestine Liberation Organization (PLO), and Lebanon. He maneuvered successfully between the Soviets and the Americans while bringing the Lebanese civil war to a close by aiding the Maronites against the PLO. But Israeli denial of southern Lebanon to him made the PLO a necessary proxy in that area, while Israeli support sent the Phalangists and Chamoun to war against the Syrians. Asad's response, a call for an eastern front, was rejected by Iraq and Jordan. Meanwhile, the Hama massacre was required to suppress the growing Sunni opposition in Syria. Israeli military intervention in 1982 evicted Syria from most of Lebanon and produced an American-backed Israeli-Lebanese treaty. Asad's health broke, and power struggles erupted. But Asad made a remarkable recovery. Unable to reconstruct an Arab front, he has maintained beneficial ties to Iran and the Soviets. He has attained near military parity with Israel and used state-run terrorism and guerrilla war to drive Israel and the United States from Lebanon. Ma'oz concludes his study with an estimate of Asad's prospects.

Ma'oz's argument, always reasonable, is very often persuasive. The socioeconomic analysis, however, is without much substantiation. And the activities of the Muslim Brothers are not proof of an overpowering Sunni-Alawi contradiction. Without considering the issues of Israeli and other aid, the Muslim Brothers

have always opposed Sunni governments in Syria and elsewhere, not just Asad's. Asad's personal contribution to his successes is exaggerated. He could have had no success in 1973-74 without the Egyptians, and his success in Lebanon since 1982 results largely from Israeli and American action based on the fantasy that the Syrian army had been defeated. But, whatever future research may reveal, Ma'oz is to be applauded. His book is a fair-minded and even-handed examination of an immensely important subject that uses all of the evidence that is likely to be accessible for a very long time.

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MAJID KHADDURI. *The Gulf War: The Origins and Implications of the Iraq-Iran Conflict*. New York: Oxford University Press. 1988. Pp. viii, 236. \$24.95.

Paragraph six of Security Council Resolution 598, one of the useful documents reproduced in this study of the gulf war, "requests the Secretary General to explore . . . the question of entrusting [sic] an impartial body with inquiring into responsibility for the conflict." The resolution was voted on July 20, 1987, and a year later became the basis for a cease-fire after almost eight years of fighting. Majid Khadduri completed his book during the intervening year when the prospects for a near-term cease-fire looked dim. It therefore reflects the final stages of the war but quite understandably does not foresee its end.

Naming an aggressor was the stumbling block for Iran in accepting the cease-fire resolution. Earlier paragraphs called for a UN-monitored cease-fire, withdrawal to international frontiers, and an exchange of prisoners. Iran feared that, once these steps were taken, the war-guilt paragraph would be ignored and Iraq would escape the legal and moral burden of its invasion in September 1980. Iraq, however, supported by the United States, insisted on the sequential implementation of the paragraphs.

After months of trying to achieve a modification of the resolution, military reverses forced Iran to accept it anyway. The period that has passed since the cease-fire has shown Iran's reservations to have been justified because paragraph six has not been implemented, in large part because of new conditions that Iraq has set on negotiations.

Observing that earlier UN resolutions eschewed the words "war" and "aggression," referring simply to "the situation between Iran and Iraq," and fearing that the world would never officially condemn Iraq's attack, Iran convened an international conference on aggression and defense timed to coincide with its acceptance of the cease-fire. At that conference, foreign scholars whom the Iranians felt appreciated the logic of their position were invited to express their views. Iraq was duly condemned by this one-sided gathering.

Khadduri's book is a similarly one-sided scholarly presentation from the Iraqi perspective. Ostensibly a historical account of the background and diplomatic aspects of the conflict, it is actually a partisan condemnation of Iran and exoneration of Iraq. In the first seven chapters, the author summarizes the history of Sunni-Shi'i tensions and of the disputes over the proper border demarcation along the Shatt al-Arab waterway. He devotes one chapter to the unfolding of the revolutionary movement in Iraq since 1958 but does not have a chapter on the Iranian revolution. The remaining five chapters deal with the diplomatic and ideological aspects of the war, explicitly excluding its military dimension.

In all of these discussions, Khadduri is at pains to present the Iraqi position in the most favorable light. No Persian language sources but dozens of Iraqi ones are cited, and Khadduri notes that the only Iranians he talked to were émigrés, presumably opposed to the Islamic republic.

The author's theme is that the Islamic Republic of Iran has reverted to the sixteenth century by "advocating Shi'i teachings and traditions rather than the principles and concepts of government laid down by the Prophet" (p. 162). Iraq, on the other hand, is "an essentially modern secular state" (p. 163). Iranian use of symbolic labels for military campaigns, for example, is "obviously intended to inspire believers . . . to recover Shi'i holy lands" (p. 112), while Iraqi use of the same technique is mentioned without comment (p. 85). Iran's harboring of Iraqi opponents of Saddam Hussein's regime stems from Iran's desire to export its revolution, but Iraq's hosting of the counterrevolutionary Iranian Mujahidin-i Khalq movement is not mentioned. Khadduri is also silent about international condemnation of Iraq's use of poison gas and Iraq's apparent shooting down of a civilian airliner carrying the Algerian foreign minister to Tehran on a diplomatic mediation mission.

Aside from evident bias, the book suffers from poor proofreading and the author's annoying insistence on transliterating all Persian and Turkish names and words as if they were Arabic. Iran's then-president Khamene'i becomes Khami'ini, and the Turkish city of Erzerum becomes Arzurum (Ard al-Rum). Yet, for all of his nationalist inspiration, the author failed to examine the map on the dust jacket, which grossly misplaces Iraq's major cities and renders the gulf in a geographically bizarre fashion.

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AFRICA

SUZANNE MIERS and RICHARD ROBERTS, editors. *The End of Slavery in Africa*. Madison: University of Wisconsin Press. 1988. Pp. xx, 524. Cloth \$47.50, paper \$14.95.

Historians without specialized interest in the subject of slavery may suspect by now that studies of slavery have

moved well into the realm of diminishing returns. The present volume (despite being an anthology) should convince them otherwise. The topic of abolition in non-Western societies has not been dealt with adequately up until now, and in this case it illuminates many broader issues concerning comparative slave systems, the political economy of Africa, and the nature of European colonial regimes. Moreover, the individual contributions are all researched, written and edited to a standard that provides both confidence in, and clear comprehension of, their findings.

Although the book offers no clear-cut answers to the broad questions about the definition and significance of slavery, the authors seem to agree on several important arguments: that African slavery can neither be identified with nor diametrically opposed to the more familiar New World plantation systems; that the situation of captured and purchased people must be understood in the context of a wide range of other dependency relationships based on descent, ethnicity, and individual contract; and that colonial rule (or modernization in Ethiopia) brought about a fairly abrupt end of the African slave trade but only a very gradual and incomplete transformation of the local social relationships into which enslaved individuals had been placed.

The introductory essay transcends the ambiguities of the specific studies less satisfactorily than might be desired, in part because one of the editors, Richard Roberts, tends toward a Marxist analysis of slavery, whereas Suzanne Miers takes a more agnostic approach. The concluding reflections by Igor Kopytoff (coeditor with Miers of an earlier collection on African slavery) offer a strong, self-proclaimed "anthropological" analysis of slavery as a social rather than an economic institution. This approach yields at least one valuable insight about the hesitancy of formally manumitted slaves to depart from their masters and thus re-create the very marginality that defined them as slaves in the first place, but, even if one accepts Kopytoff's vision of a relative stasis in the slavery relationship, he must be faulted for ignoring the historical factors explaining both this situation and the more dynamic appearance of large-scale slavery in many parts of Africa.

Roberts, in his own substantive chapter on the western Sudan, and the teams of Raymond Dumett and Marion Johnson on the Gold Coast (present-day Ghana) and Jan Hogendorn and Paul Lovejoy on northern Nigeria deal with three of the more dynamic cases in very different terms. Roberts considers the exodus of hundreds of thousands of former slaves (while a million or more stayed in place) a "revolution" linked to "the implementation of capitalist relations of production" (p. 302). Dumett and Johnson, on the other hand, specifically refute Marxist historians of their own region and describe abolition here as "one of the quieter social evolutions of the late nineteenth and early twentieth centuries" (p. 108). The study of northern Nigeria notes considerable tension around the ending of slavery but a happy resolution through the

rapid development of peasant cash-cropping as a means of both occupying manumitted slaves and assuring government revenues. The common denominator in all of these cases is that colonial officials feared social and economic disruption if they acted decisively against slavery and were happily surprised to see that the kind of minimal effort they favored brought both change and relative prosperity. Roberts concludes, correctly, that this outcome shows the limited control that European administrators had over African social relations, but he (and the others) might have inquired further as to how important slavery itself (as opposed to the forced movement of cultivators and artisans into areas of greater market opportunity) really was to African economic development. All of the authors do point out the absence of extensive wage employment as an alternative to slavery in the colonial era.

It is impossible in a brief review to do justice to the remaining twelve case studies in this volume, but it should be noted that not all focus on political economy. Thus, Allen Isaacman and Anton Rosenthal discuss a group of essentially military slaves, the *Chikunda* of Mozambique, who were both the enemies of the expanding colonial state and its collaborators as police recruits. Lee V. Cassanelli also provides an excellent account of the alternatives (independent villages, clientalism, and forced colonial labor) for former slaves in Italian Somalia, and Thomas J. Herlehy and Rodger F. Morton trace an ethnically mixed community of mission-educated former slaves through the colonial history of Kenya. Finally, Linda M. Heywood describes an Angolan colony whose Portuguese rulers maintained a coerced labor system much harsher than previous African slavery.

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ROBERT HARMS. *Games against Nature: An Eco-cultural History of the Nunu of Equatorial Africa*. (Studies in Environment and History.) New York: Cambridge University Press. 1987. Pp. xi, 276. \$42.50.

In this innovative study, Robert Harms reinforces his position as a leader in the small but important group of historians laboring to reveal the social dynamics of politically decentralized populations of the Central African forest. His presentation of how to do this sort of history is virtually as important as the historical analysis itself. The hallmarks of this study are the collection of apparently unrelated oral traditions, which are woven into a social-historical narrative through study of the region's ethnography and ecology, and presentation of the narrative through the device of game theory. Harms shows the Nunu, a small group who have fished the small tributaries east of the Zaire River for the past several centuries, to have experienced successive waves of displacement, differentiation, and transformation.

Harms uses game theory more to characterize the

strategies of various participants than to follow specific games to their conclusion. Nevertheless, he adds a historian's twist to game theory, by showing how each game, when played over time, led to creation of a new situation and to initiation of a new game. In so doing, Harms makes the reader aware of the game he has played as historical analyst: drawing a chronological narrative (though in relative not absolute time) out of several types of cross-sectional data. The reconstruction is remarkable in that the past that Harms retrieves is dramatically different from Nunu society of today.

The founders of the Nunu came down river from the north. They planted *nkinda* charms and built dams and ponds on small streams, in mixed environments, in flooded forest, and in floating prairie. They became water lords—owners of fishing estates—and their strategies focused on accumulating money and labor obligations. For the landless, strategies centered on migration. By the nineteenth century, the best fishing lands were occupied, and migrants headed in two directions. First, they moved east to the dry lands of Nkuboko, where they had to supplement fishing with hunting and agriculture. Second, they moved west and south to the river, where they founded the settlement of Bolobo and became river nomads.

Harms shows the differentiation of the common culture as people had to accommodate to distinct local ecologies. At the same time, he shows the links of the various Nunu settlements into a regional system both through marriage and lineage ties and through the wars that grew up among settlements as a form of ritualized competition. Surprisingly, the Nunu were so enmeshed in the local dynamic of their own history that they retained little memory of the Atlantic slave trade that dominated the history of their Bobangi neighbors on the Zaire River.

Into this system intruded Henry Morton Stanley, the Belgians, and, eventually, national independence. Harms traces the transformations skillfully, although he does not adjust his framework to account for them. The motor of change is more often outside impact rather than internal evolution, and the dynamic of culture and nature is overshadowed by that of culture and the state. New fishing nets and outboard motors join other factors in pushing the Nunu to populate the two riverine settlements of Yumbi and Bolobo; in ritual terms, the *nkinda* charms are replaced by mission stations. As regional and national politics developed, most Nunu threw in their lot with the Bobangi of the river and assumed a new identity.

The book is written in a charmingly terse style that appears to reflect both the proclivities of the author and the condensed recollections of his Nunu informants. It is inevitably speculative, but it makes the forest come alive.

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J. MUTERO CHIRENJE. *Ethiopianism and Afro-Americans in Southern Africa, 1883–1916*. Baton Rouge: Louisiana State University Press. 1987. Pp. xii, 231. \$27.50.

When J. Mutero Chirenje asserts that "white society in South Africa continued to believe that the United States fostered political discontent among black South Africans that in time could well lead to a revolt against white rule," his reference is the first decade of the twentieth century, although it might apply equally to our own day. Indeed, Chirenje's monograph, a brief survey of an "almost mystical connection" between Africans of the diaspora and those on the continent in the years around the turn of the century, reveals a quite deliberate effort to parallel the church independency movement of those early years with the more recent "armed struggles for national liberation." Readily accessible and gracefully written, the book succeeds within the context it establishes, that is, as a study of a liberation effort framed as a religio-political movement.

Chirenje tells the story of the emergence of black church independency, known as Ethiopianism, in southern Africa in the 1890s. Ethiopianism, the author reminds us, became a generic term to describe a movement that was political, educational, and religious in intent. The Reverend Mangena Maake Mokone, the pioneering seceder from white religious domination, left his Wesleyan Methodist connection, located near Pretoria, in 1893; by 1896, after other clergymen followed his lead, the Ethiopian Church of South Africa was able to affiliate with the African Methodist Episcopal Church (A.M.E.) in the United States. A major catalyst in promoting the relationship between the two independent religious bodies was A.M.E. bishop Henry McNeil Turner, an ardent advocate of pan-Africanism and black missionary activity. In 1900, the A.M.E. denomination appointed Leroy J. Coppin as the first resident A.M.E. bishop in South Africa.

Although African Methodism exerted the greatest attraction for the Ethiopian secessionists, it was not the only draw. The National Baptist Convention found supporters, as did independent ecstatic prophets and messiahs whose message, Chirenje tells us, was concerned with "a millennium of bliss." White leaders in South Africa were frightened by the potential suggested by Ethiopianism and between 1903 and 1905 established a commission to investigate what appeared to their eyes as a unified effort. The "witch hunt" concluded that this religious flowering was not a subversive movement.

Chirenje's discussion, which offers a welcome new perspective on African Methodism (as well as on Booker T. Washington and Tuskegee), is essentially concerned, however, with the African consequences of Ethiopianism. The church independency movement, he maintains, was the outgrowth of a desire to demonstrate black leadership and led to specific gains in education, with the creation of Fort Hare University College in 1916, and political determination, with the

formation of the South African National Congress (A.N.C.). "Freedom," he concludes, "was what Ethiopianism was all about."

Chirenje draws on archival sources found in southern Africa, Britain, and the United States, although he relies heavily on the periodical literature, in particular *Imvo Zabantsundu*, a black African secular newspaper that endorsed self-help, and the *Voice of Missions*, started by Bishop Turner. The secondary evidence, books and articles, is gathered from the 1970s, with only two or three sources dated as recently as 1980–82. One might wish that Chirenje had taken the religious dimension in his religio-political equation as seriously as the subjects of his work obviously did, and there are distinguished models in studies by scholars such as Raboteau and Sobel. He notes in passing the seceders' desire to blend Christianity with African culture, but unfortunately we are told little more about their theology, rituals, or folk beliefs. But his brief account does develop another story that needed to be told, namely, the important affectional link, forged early in this century, between black coreligionists in America and southern Africa.

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ASIA

JOHN KNOBLOCK, editor. *Xunzi: A Translation and Study of the Complete Works*. Volume 1, books 1–6. Stanford: Stanford University Press. 1988. Pp. xvi, 340. \$55.00.

This is the first of a projected three volumes of studies and annotated translations of all of the extant writings of Xunzi, or Hsun Tzu, as he is still known in the Wade-Giles system of romanization. John Knoblock's announced aims are to provide a translation that scholars who read Chinese can use with his citations from the commentarial tradition, which justify his readings and emendations, and provide interested others with sufficient explication and contextual material for them to make sense of Xunzi's arguments as translated and described.

Knoblock succeeds in the first aim. H. H. Dubs's attempt at a translation of nineteen of the thirty-two chapters, published in 1928, cannot compete. Burton Watson's serviceable translation of ten chapters has held the field for a quarter of a century, but Knoblock's Sinological standards and attention to philosophical argument are clearly to be preferred. His English translation will become the standard, once his project is completed. (In volume 1 there are fewer than fifty pages of translation, consisting of the first six chapters, or books as he calls them.)

In his introductory chapters, Knoblock sets forth his interpretation of key terms, some of which may be questioned. For example, he declares that "the term that the Chinese use to refer to Nature is *tian*" (p. 67), which leaves the impression that a complicated concept with a long tradition in European languages had an

early Chinese analogue. In his translations in volume 1, Knoblock usually renders *tian* as "sky" or "heaven," so his gloss of it as "nature" does not intrude. He explains *li* as pattern or principle of order, which is generally acceptable, but then he declares that *li* plays a role similar to that of "*logos* or reason" in what he refers to as Western philosophy (p. 80). In the translations and discussions, the terms "reason" and "rational" are introduced gratuitously; for example, *li* shows up as "reasoned order" (p. 227) and "perfect logic" (pp. 223–24). This practice seems to me likely to mislead readers who have no recourse to the Chinese text rather than to make early Chinese philosophy more reliably accessible, which is Knoblock's second aim.

Knoblock's contextual material will serve not the imagined "general reader" but scholars of China who want to read about Xunzi but may not have the time or skills to do the Sinological spadework that Knoblock has done so ably. He has assembled the evidence to support a conjectural biography of Xunzi that is admirable in locating him in contemporary political events. Knoblock argues for shifting Xunzi's dates to 310–210, or thirty-five years later than the conventional ones endorsed by Ch'ien Mu (and ironically still required by the Library of Congress cataloguing-in-publication data). Knoblock gives a meticulous history of the text and its commentaries, as well as his considered judgments on doubts about the authenticity of certain segments. His economical summary of the intellectual milieu will bring some order for his readers to the "astonishing variety" (p. 66) of the Hundred Schools. He dutifully adds an appendix reviewing the old evidence and arguments over whether Sun or Xun was the correct form of Xunzi's family name.

Not everyone need assent to everything Knoblock has presented in volume 1, but, as it is joined by the next two volumes, everyone will agree that he has produced a first-rate work of scholarship on a central philosophical text.

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ELIZABETH ENDICOTT-WEST. *Mongolian Rule in China: Local Administration in the Yuan Dynasty*. (Harvard-Yenching Institute Monograph Series, number 29.) Cambridge: Council on East Asian Studies, Harvard University and Harvard Yenching Institute, Cambridge; distributed by Harvard University Press, Cambridge. 1989. Pp. 217. \$23.00.

This is not as broadly based a study as its title suggests. It is, rather, a study focused specifically on the nature and evolution of the functions of one type of office, the *darughachi*, in the Mongol government of China in the thirteenth and fourteenth centuries. The word means an official seal holder or commissioner. It was one of a number of administrative innovations that the Mongols introduced into China. Early on, while the Mongols were still making their great conquests, the *darughachi*

were unsalaried appointees with hereditary rights to the office who were charged by the khan or some other Mongol leader with one or more of a range of local tasks in conquered areas. From the time of Qubilai (r. 1260–94), the Mongols' conquests in China were, at least from the Chinese point of view, "normalized" into the Yuan dynasty, and the *darughachi* offices were transformed in the process.

Just what those offices became in Yuan times is hard to say, however. The Mongols featherbedded the Yuan bureaucracy, in part to create overlapping responsibilities for purposes of control but also, surely, to provide jobs for needy and deserving Mongol elites. Much of this inflation took place at regional and local levels. *Darughachi* posts were established at every regional and local level of the civilian, military, and appanage administrative hierarchies. Although here and there individual *darughachis* were forceful and effective, Elizabeth Endicott-West argues that on the whole their role does not appear to have been essential to the Yuan system of rule in China. The *darughachis* were required to act in conjunction with other local officials, some of equal salary and rank, in a collegial or "conciliar" system of administration and adjudication. Conciliar procedure was familiar from the princely *quriltai* (assembly) of early Mongol history, but it was an alien implant in China. Nevertheless it worked, although in the author's opinion it worked neither very efficiently nor very well. Indeed, after the end of the Yuan, the succeeding Ming dynasty (1368–1644) from the outset abolished all councils and *darughachis*.

This is a technical, philologically exact, densely footnoted study. Interspersed throughout are literal translations of selected documents relating to *darughachis*. Many of the original sources are in a notoriously difficult chancellery style peculiar to the Yuan period. The translations are in a style of English that strives for exactitude but is often impenetrable or unreadable as a result. One monster of an example (pp. 69–72) really needs some plain-language exegesis.

Despite the difficulties, Endicott-West does make a new and original contribution. We now have a better idea than we had of the ethnic origins, tasks, and hereditary rights of *darughachis*. Their role in a system of local government councils has never been so well shown as it is here. The author also demonstrates that, contrary to some careless assertions, the *darughachi* clearly were not special representatives of central authority. But, because their roles as they evolved were not, as she shows, critically important to Yuan rule and because their authority was not distinct from that of various other local officials, they are just not quite as revealing of the whole Yuan system as the author would like to assert (p. 3). How Yuan local government interacted with local society, and local society with it, and how centralized or decentralized the Yuan system of government was are questions that need to be addressed from some broader point of view and on the basis of some deeper and wider research. One hopes that Endicott-West will continue to write about these

problems. Very few scholars command the level of expertise such research requires.

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FREDERICK W. MOTE and DENIS TWITCHETT, editors. *The Cambridge History of China*. Volume 7, *The Ming Dynasty, 1368–1644, Part 1*. New York: Cambridge University Press. 1988. Pp. xix, 876. \$110.00.

The first of two volumes of the *Cambridge History of China* devoted to the Ming dynasty (1368–1644), this book is the only substantial account of the period written in English. Because of the lack of modern secondary works on Ming history, extraordinary measures were required to make this volume possible. Under the leadership of coeditors Frederick W. Mote and Denis Twitchett, a series of summer workshops were held at Princeton University to facilitate basic research in the traditional sources and to draft the chapters. This volume, part 1, gives a survey of the political history of the Ming and provides research aids to support future scholarship; a second volume of topical essays (part 2) will be published later. The first eleven chapters of this work provide a chronological survey of Ming history that reflects accurately the state of current knowledge of the subject: clearer and more detailed in the early and later years, cloudier and more impressionistic in the middle.

The first three chapters go over the founding of the Ming dynasty from these perspectives. Chapter 1, by Mote, describes the emergence of the Ming regime from the chaotic strife of the late Yuan, a refined version of a story told earlier in his work on the poet Kao Ch'i. In chapter 2, Edward L. Dreyer goes over roughly the same time period as Mote from the perspective of the military origins of the Ming, a topic on which he has published. John D. Langlois, Jr.'s account in chapter 3 is one of the richest and most enlightening in the book, carrying the story forward through the thirty years of the first reign. Adding fresh insights on ideology, religion, ritual, and institutional change, Langlois makes effective use of surviving pronouncements and codes of the founder, Chu Yuan-chang, one of the greatest legislators (and tyrants) in all of Chinese history. Hok-lam Chan's account of the four reigns from 1399 to 1435 is easily the longest in the book at 122 pages. Chan does a masterful job of weaving together the story line without losing sight of the problems of sources and terminological changes that conspire to make the period confusing.

That chapters 5 through 8 are written by the volume's two editors and the coordinator of the Ming History Project illustrates the absence of established scholars working in the middle period of Ming history. The content and tone of these chapters are necessarily closer to traditional dynastic histories than are those on periods that have received more study. Chapter 5 on the years from 1436–64, coauthored by Twitchett and

Tileman Grimm, is the briefest and most disappointing in the book. Mote's chapter 6 tells us more about the palace than about changes in the military system, revolts, foreign relations, and border problems, which are all duly noted. It is still too early to place these topics in a clearly understood social and economic context. James Geiss's recounting of the Cheng-te (1506–21) and Chia-ching (1522–66) reigns, in chapters 7 and 8, provides coverage of the early sixteenth century not available elsewhere but is narrowly focused on the palace and tends to be overly anecdotal.

The tone changes sharply in chapter 9, devoted to the Lung-ch'ing and Wan-li reigns, 1567–1620, and authored by Ray Huang, who, operating on familiar ground, takes a long view and provides lively accounts of Chang Chu-cheng's reforms, the Tung-lin reaction, and the rise of the Manchus. Chapter 10 by William Atwell and chapter 11 by Lynn Struve give us solid accounts of the dynasty's fall and loyalist remnants from the hands of established scholars working in well-known territory. Atwell's speculations about climate, demographic change, and silver flow provide the volume's first glimpse of China in a world context.

Because of the underdeveloped state of modern research on Ming China, this book will become the most important single source of information on the topic for decades to come. The editors' understanding of this critical role is indicated by their devotion of more than a quarter of the volume to reference materials. Beginning with chapter 12, Wolfgang Franke's discussion of historical writing during the Ming, there are two hundred fifty pages of valuable research aids. Following the text proper there is a lengthy set of bibliographical notes for each chapter, a general bibliography, and a glossary index; the bibliography and index are made invaluable as handy finding lists by the inclusion of Chinese and Japanese characters following the romanized forms of names, terms, and titles. Noteworthy among the reference aids are an excellent genealogy of the imperial family and a map of the empire that, by resorting to very small type, manages to identify every prefecture. Most unfortunately, a type-setting error in a table of Ming emperors has misaligned the temple names of all but the first three emperors. The confusion that this error may cause is doubly grave, because of the need, first, to make frequent reference to the chart to keep straight the correspondence of personal name to reign name and temple name and, second, because readers will naturally turn to this volume as an authoritative reference for matters of chronology.

The editors and authors of this monumental work are to be congratulated for a heroic accomplishment. With its appearance the whole field of Ming history has taken a long stride forward. Now we would like to see part 2.

EDWARD L. FARMER
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ARIF DIRLIK. *The Origins of Chinese Communism*. New York: Oxford University Press. 1989. Pp. xiv, 315. Cloth \$35.00, paper \$12.95.

So much has been written about the origins of communism in China that historians have tended to assume that this event has been understood. Arif Dirlik challenges that complacency in this book. This sophisticated study draws on knowledge accumulated in previous works, and Dirlik particularly stresses his debt to Maurice Meisner and Benjamin Schwartz. Nonetheless, he reorients our perspective on the establishment of the Chinese Communist party (CCP) and the beginnings of Marxism in China. Already known as a student of Marxism, Dirlik broadens his inquiry here to encompass other strains of socialism in the early Republican period and to examine the interplay of ideology, organization, and radical activism. The resulting account is finely textured and filled with insights.

The standard historiography sees the Russian revolution of 1917 as stimulating radical intellectuals' interest in Marxism. Nationalistic frustrations, unleashed in the May Fourth movement of 1919, expanded the appeal of Leninist ideology, and Comintern advisors subsequently aided a small group of Chinese Marxists to establish the party in 1920. Dirlik does not reject these main outlines but argues that they oversimplify history, and he adds a detailed social context based on a variety of radical periodicals and accounts of May Fourth associations. He maintains convincingly that there was no straight line of development, and the triumph of Bolshevism over other forms of Marxism was initially far from obvious.

The complexity of Dirlik's views defies brief summary, but several points stand out. First, socialism was an important element in the ideology of the New Culture movement of the mid-1910s, but this socialism derived from anarchism, not Marxism. Anarchism linked the desires of youth and women for individual freedom to social concerns, thereby helping radicalize younger intellectuals. It also corresponded to what Dirlik calls the "elite voluntarism" of the New Culture movement, and Chinese anarchists' revolutionary views had an ethical, educational, and reformist flavor that resonated with the "confidence in immanent cultural metamorphosis" of that period (p. 117).

Second, the industrial expansion during and after World War I resulted in the highly visible emergence of capital and labor in major cities, enhancing the relevance of Marxism to Chinese radicals. This indigenous socioeconomic development contributed to the growing preoccupation with socialism and politics at the end of the 1910s. May Fourth radicalism was thus not only the result of anti-imperialist nationalism, and admiration for the then rapidly expanding labor movement prompted radicals to accept class politics around 1920.

Third, the May Fourth socialism was eclectic, and radicals still generally favored nonviolent revolution, preferring scientific Marxism to class struggle. Guild

socialism, the reformist state socialism of such old Guomindang members as Dai Jitao, and humanistic anarchism all had significant followings. Fourth, the May Fourth period shaped the organizational and political context in which the CCP was established. The proliferation, splintering, and regrouping of study societies produced personal networks and initiated a dynamic of radical activism forging radical identities and loyalties. At the same time, radicals were driven to despair as the social programs of their little associations failed and governmental repression increased in late 1919–20. The establishment of the CCP was the direct result of Comintern intervention in China, but the success of its representative, Girigorii Voitinsky, depended on the existence of this favorable Chinese context.

Finally, the establishment of the CCP initiated a new and more authoritarian organizational dynamic in which discipline was imposed on Chinese party members. By 1922, being a communist meant accepting the Bolshevik version of Marxism and the party's political line. This ideological narrowing and dictatorial discipline was both a legacy of early Comintern dominance of ideologically naive Chinese radicals and the product of the political conditions in which the CCP had to compete during the Republic. But Dirlik also suggests that party authoritarianism perpetuated itself after the original justifications disappeared.

The book is somewhat abstruse, sometimes repetitious, and inevitably cannot answer all questions about radical politics and ideologies during the formative period from 1915 to 1922. Nonetheless, it more than repays the reader's effort and does much to set future research agendas, which might include further examination of the ideology and organizations of a diffuse radical social movement, serious consideration of the interpenetration of liberal and socialist values in this period, and exploration of Dirlik's very fruitful ideas on the interplay of ideology, organization, and social activism.

Although Dirlik is careful not to jump ahistorically forward from the 1920s, the appearance of this book at a time of intense questioning of the legitimacy of party leadership and the validity of Marxism in China cannot but increase its relevance to the present. Both Chinese and Westerners might well consider his picture of a more open, democratic, and humanistic socialism prematurely suppressed during the Republic. With the passing of justifications for revolutionary dictatorship, the submerged anarchist and other socialist strains might provide a path to democracy within the heritage of twentieth-century Chinese radicalism.

MARY BACKUS RANKIN
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JERRY DENNERLINE. *Qian Mu and the World of Seven Mansions*. New Haven: Yale University Press. Pp. xiv, 192. \$22.50.

This is an unusual book, a sensitive, learned, and original attempt to place a modern Chinese intellectual firmly in the local society and culture out of which he came. Qian Mu (1895– ; Ch'ien Mu in the older romanization) is a leading twentieth-century historian and classicist. A professor at Beijing University in the 1930s and, after 1949, founder and president of New Asia College in Hong Kong, he has written (in Chinese) dozens of influential books on Chinese history and philosophy. He is also a cultural conservative who believes in the continued relevance of China's history and traditional values. How little Westerners have been interested in Chinese cultural conservatism (with its implicit or explicit rejection of Western values) is suggested by the lack until now of scholarly work in English on Qian Mu.

Jerry Dennerline is concerned not with summarizing Qian Mu's voluminous writings but with excavating the roots of his moral vision: the world that molded him and his attachment to the common values, based in local society but shared all over China, that (Qian thinks) animated Chinese history and unified Chinese culture. In the first of the three major parts of the book, Dennerline surveys Qian's life, his ideas, and the currents of his time. This is mainstream intellectual biography, well done—Dennerline is a seasoned and accomplished scholar—though a little too brief and lofty a survey to provide much excitement for specialists.

The second part is meatier, a social historian's exploration of the local world of "Seven Mansions Village," Wuxi, not far from Suzhou (Soochow), where Qian Mu was reared in an impoverished branch of a literati family. In part Dennerline is skillfully synthesizing the growing body of work on Chinese social history, drawing on his own considerable expertise on local history of the lower Yangtze River. But he has also added much concrete detail from interviews he conducted with local people there in 1985–86. Behind gracefully written descriptions of land tenure, lineages, hereditary dependents, popular customs, religious beliefs, and the place of women lies a concern with the moral world of local society: how economic inequalities and an exploitative land system were tempered by the charitable estates of wealthy lineages; how traditional habits of mutual obligation humanized relations between landowning families and hereditary dependents; and how, within the family, a woman's economic weakness and submissive role were counterbalanced by her moral authority.

Finally, Dennerline presents us with his translation of Qian's reminiscences of his family and childhood written in Taiwan in 1975 when he was eighty. This short (thirty-three pages in translation) and simple text, describing in particular the kind, dignified, and scholarly father who died when Qian Mu was eleven, now takes on vividness and meaning because of the rich context that Dennerline has provided.

This book is, then, intellectual biography plus local history plus family memoir. Does it all come together to

explain "what made the *li* [rites] believable" (p. 10), the appeal of the values that have united Chinese society over the centuries? I think largely it does. The three-directional approach in the end gives us something of a feel for how the "deeply felt harmonies of the old culture—familial love, respect for learning, intellectual integrity, political detachment, moderation in personal and public affairs" (p. 6)—were rooted in sense of place and "human bonding through family roles" (p. 114).

Dennerline finishes with a concrete and thoughtful discussion of the extent to which this traditional spirit of Chinese civilization is now being lost through the changes that are occurring in local society: rural industry, which is more individual and less dependent on the family than agriculture; universal education that is technical not moral; the decline of religious practices, especially ancestor rites; and the rise of television as the new cultural unifier.

R. DAVID ARKUSH
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JOSHUA A. FOGEL. *Life along the South Manchurian Railway: The Memoirs of Itō Takeo*. (East Gate Books.) Armonk, N.Y.: M. E. Sharpe. 1988. Pp. xxxi, 241. \$29.95.

Itō Takeo was Japan's "China specialist" in the pre-World War II era. As a student at Tōdai, he was inspired by the writings of Yoshino Sakuzō, an intellectual spokesman for Taishō democracy and a prominent scholar on modern China. Itō also became a member of the Shinjinkai (New Man Society), a liberal study group founded by Tōdai students.

Ironically, Itō, an admirer of Taishō democracy, joined the research department of the South Manchurian Railway (SMR) upon graduation from the Tōdai in 1920. The SMR served as a vehicle for Japan's colonial expansion into Manchuria from its acquisition by Japan in 1905 to the end of World War II. The memoirs of Itō Takeo include special reference to his association with SMR.

In the prewar years, many Japanese were attracted to the opportunities available in Manchuria and went to work for the railway. Among them, some were adherents of a right-wing ideology, which saw Manchuria as Japan's lifeline. They supported Japanese military expansion when the army rose to power in the 1930s. There were also a number of liberals like Itō, however, who joined the SMR for different reasons. In fact, some of them were socialists or communists.

Why would the liberals in Japan go to work for a colonial company such as the SMR? Itō explains that, although these people were still "anticolonialists" in thought, they had no choice but to look for opportunities in Manchuria, as the government began to crack down on them at home. Moreover, their research skills in social sciences were needed by the SMR.

Itō's rationalization remains unconvincing. It does however, point to the passivity and the fundamental weakness of prewar Japanese liberalism. The liberals

had experienced the intellectual trends of Taishō democracy and believed in certain reforms of the Japanese political system. Yet they were reluctant to take strong measures against the government. Furthermore, insofar as China and Manchuria were concerned, the liberals had virtually acquiesced in Japan's expansionist policies. It can be argued that the prewar Japanese liberals were in fact "closet imperialists."

The bulk of Itō's memoirs traces his research activities dealing with the history, politics, and society of China. The objectivity of these studies may be questioned, but certainly they provide a valuable source of information for students of modern China.

The last chapter of Itō's memoirs includes his statement of introspection. Overwhelmed with a sense of guilt at Japan's aggression in China and the failure of the liberals to counter the expansionistic policies of the military, Itō devoted his time and energy after the war to the promotion of Sino-Japanese friendship. He became an admirer of the People's Republic of China and an apologist for its actions.

B. WINSTON KAHN
Arizona State University

MICHAEL WEINER. *The Origins of the Korean Community in Japan, 1910–1923*. (Studies on East Asia.) Atlantic Highlands, N.J.: Humanities. 1989. Pp. 249. \$39.95.

Most people are aware that Koreans in Japan suffer from discrimination. Michael Weiner argues that the discrimination that the Japanese inflict on Koreans is based on prejudice firmly established by the middle of the 1920s and based on stereotypes that began in Japan's experience with Korea during the Meiji period (1868–1912). To wit, Koreans were initially viewed as defective in character: weak, lazy, ignorant, lacking the spirit of enterprise, and corrupted by the maladministration of the late Yi Dynasty (1392–1910). Although those familiar with the record of Koreans in China, Russia, and Hawaii at that time know that these images of the Koreans were and are false, the fact remains that the Japanese thought as little of the Koreans as President Theodore Roosevelt did when he raised no objection to the Japanese takeover of Korea at the turn of the century.

After Japan annexed Korea in 1910, its land policies displaced many peasants from the southern half of Korea. Their unemployment, combined with the increased demand for labor in Japan during World War I, led them to emigrate to Japan where they were employed in mines and textile mills. Those Koreans who worked in the mines were identified with outcast Japanese groups (*eta*) who were also used in the mines, thus reinforcing a negative image of Koreans by Japanese. The Koreans were largely illiterate displaced workers pulled by the higher wages in Japan and pushed by their own poverty, and they exhibited the classic *dekasegi* characteristics of sojourners, failing to sink roots and generally living unsettled lives. Thus,

they received lower wages for the same work and were housed in squalid conditions. Koreans thus were seen up close by the Japanese as unsavory people who were a source of crime and disease.

Although Korean workers remained largely indifferent to labor and socialist movements because of their sojourner status, Korean students in Tokyo did not, occupying the extreme Left of the political spectrum. Many were influenced by Japanese liberals and socialists such as Yoshino Sakuzō and Katayama Sen. When the March First Korean nationalist movement of 1919 broke out, Korean students in Japan were actively involved, and many turned to the Soviet Union and Marxism because of the disappointing response of the Western democratic capitalist countries. Their affinity for these leftist ideologies also added to the Japanese animus against Koreans.

The culmination of Japanese prejudice is recounted in the sixth and final chapter on the Kanto earthquake of 1923. Weiner concludes that the police spread unsubstantiated rumors that Koreans were taking advantage of the disorder in the aftermath of the earthquake and recommended that vigilante groups (*Jikeidan*) be formed to protect communities from these "marauding" Koreans. As a result, several thousand Koreans were killed either in police custody or by vigilante groups. Only a handful of Japanese were ever brought to trial for killing Koreans, and those few convicted were quickly pardoned. Weiner marshals evidence to suggest that the government spread the rumors to create a pretext for the declaration of martial law to prevent a repetition of the rice riots of 1918. Many of these government officials had served in Korea, and they harbored negative views of Koreans. Thus, the treatment of Koreans was truly the logical outcome of Japanese colonial policies and ideology.

Weiner is clearly at home in his extensive use of Japanese documentation. Although he did not use Korean-language sources (and there are a few errors in the bibliography listing Korean authors), his findings are similar to those of Michael Allen who does use such sources in a forthcoming piece on the Kanto earthquake. In addition, Weiner's argumentation is logically and carefully developed. In sum, he has produced a compact and useful work that is a valuable addition to the literature on Japanese-Korean relations and Koreans overseas.

WAYNE PATTERSON
St. Norbert College

GREGORY J. KASZA. *The State and the Mass Media in Japan, 1918-1945*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xvi, 335. \$35.00.

Japan before 1945 boasted a sophisticated network of mass media comprising national and local newspapers, magazines, radio broadcasting, and cinema. Japan also possessed one of the most pervasive systems of censorship, which reached its peak during the war years. This

thoroughly researched and thought-provoking monograph analyzes the ways in which the Japanese state controlled the media during the period between the end of World War I and the end of World War II. It is an interesting case study of what Gregory J. Kasza calls the unprecedented expansion of the power of states over their subjects in the twentieth century (p. xi).

The book is divided into two parts. Kasza first focuses on the years 1918-32, when Japan enjoyed a limited degree of democracy and liberalism. In the second part of the book, he focuses on the years 1937-45, when a military-bureaucratic elite, taking advantage of the wartime circumstances, attempted to restructure Japan into a centrally planned and administratively controlled state. Each section has separate chapters dealing with the press, broadcasting, and the film industry, and each section is followed by a comparative analysis that puts it in a broader perspective, relating the Japanese case to similar phenomena in other countries.

One of Kasza's most interesting findings is that Japan's party cabinets during the twentieth century imposed harsher censorship than had the oligarchs during the nineteenth century and that press controls were therefore more stringent during the Taishō era than they had been during the preceding Meiji era. Nevertheless, Kasza contends that control of the media in Japan before 1937 was not very different from that in Britain and France at the time. In both Western Europe and Japan, bureaucratic policy makers initiated legislation to curb the content of newspapers and broadcasts and did so with the full support of elected politicians.

As Kasza shows, during the late 1930s and early 1940s, the Japanese press and film industry were, like the German, among the most advanced in the world. Yet, unlike the Nazis, the Japanese government did not need to rely on brute force in order to obtain compliance from its subjects. The government's moral authority in wartime was usually enough to achieve that goal. As in fascist Italy, a high level of mobilization was attained in Japan without recourse to extensive coercion.

Kasza dismisses the widespread assumption that the wartime years constituted an abnormal interval detached from the periods that preceded and succeeded it. He claims that the new order of those years, established to mobilize the people for the war effort, was one of the most formative turning points of modern Japan, together with the Meiji Restoration and the American occupation. Thus, the wartime labor mobilization was the basis for today's enterprise-based union system, and the postwar power of the farmers' national association of agricultural cooperatives (*Nōkkyō*) is built on the foundations of the wartime structure of agriculture. Similarly, some of the present characteristics of the Japanese press, such as the ownership by their staff of the three large daily newspapers, the *Asahi*, *Yomiuri*, and *Mainichi*, were introduced by the wartime reforms.

Kasza shows that, although the degree of state con-

trol over the content of newspapers in wartime Japan equaled that in Nazi Germany, the effectiveness of control was greater in Japan. Without using the coercive methods of the Germans, the Japanese bureaucrats produced a propaganda network more effective than that of Joseph Goebbels in molding public opinion.

Rather than viewing the military-bureaucratic regime of wartime Japan as a digression from modernity, Kasza presents it as a normal, albeit unpleasant, stage in the history of a developing country, similar to Gamal Abdul Nasser's Egypt during the years 1952–70 and Velasco Alvarado's Peru during the years 1968–75. In all of those countries, newly risen military regimes tried to carry out statist revolutions by imposing stiff controls on what citizens were allowed to read, hear, and watch, and they imposed controls out of a shared conviction that it was the best way of mobilizing the masses for the modern reforms.

BEN-AMI SHILLONY
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KIYOKO TAKEDA. *The Dual-Image of the Japanese Emperor*. Foreword by IAN NISH. New York: New York University Press. 1988. Pp. xvi, 183. \$34.50.

Kiyoko Takeda is a leading Protestant layperson who has served as a president of the World Council of Churches. This volume is mainly an adaptation of a book published in 1978, with two chapters at least based on more recent research. Only a handful of the two hundred notes, however, are to works published after 1970. The adaptation and translation are the author's (the translation with the assistance of Kayama Shinji and M. William Steele). On the whole, the text reads smoothly, aside from annoying inconsistencies with names (the author's name is in Western order; Kayama's, in Japanese order) and dates (Ian Nish gives the date of publication as 1970 [p. x]) and some obvious typographical errors (Yoyoma for Toyama on page 35).

The subject of the book is "the problem of the dual nature of the Japanese emperor" (p. 4), a more felicitous phrasing than the book's title. More precisely, the subject is Allied views of that dual nature during World War II. Five chapters sketch views of the emperor held by the U.S. State Department; the British; the Institute for Pacific Affairs (IPA), which Takeda identifies as "a Canadian view"; the Australians; and the Chinese; two final chapters discuss the surrender negotiations of 1945 and the Occupation. Then come a brief conclusion (4 pages), an epilogue (4 pages), and an appendix (4 pages).

The chapters on national opinions are not quite as clear-cut as the categories suggest. Only four pages (of seven) in chapter 1 discuss State Department views; the chapter on the IPA includes nearly ten pages on American views and two pages of biographical data on E. Herbert Norman; the chapter titled "The Chinese

People and the Military Expansionism of Japan" focuses on Kuomintang views before turning—for lack of public statements by the Chinese Communist party—to the thoughts of Nosaka Sanzo and Gunther Stein.

Little of what Takeda discusses is startlingly new, but her canvas is still useful. Takeda notes the change in the early postwar years from *kokutai* (polity) to *tennosei* (emperor system): "the term 'emperor system' which had been used primarily by the communists in a negative sense gradually came to be used in place of 'national entity' or 'national polity.' Even the defenders of the idea of Japan's unique *kokutai* came instead to use the term 'emperor system.' . . . In this way 'emperor system' became the generally accepted term, taking on roots in postwar Japan" (p. 113). Takeda's own political commitments become clear early on (p. 5): "It is the duty and responsibility of the government of any democratic nation to eliminate the visible and invisible pressures which instill fear and force certain patterns of thought and behaviour on the people. The government should not initiate or encourage any policy which . . . strengthens such reactionary directions or ideologies." She welcomes the Occupation: "the Occupation forces were indeed an 'Army of Liberation' for a large majority of Japanese" (p. 111). She calls on the Japanese people: "authoritarian and inhuman aspects of the other side of the 'dual image' of the Japanese emperor still remain part of the Japanese 'mental structure' even today. . . . In the future the Japanese people can and must play a more positive and more central role in the further liberalisation and humanisation of their country" (pp. 152–53).

In this year of Hirohito's death and Edward Behr's hatchet job ("Hirohito: Behind the Myth"), Takeda's essay is both poignant and reassuring.

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ANDRE WINK. *Land and Sovereignty in India: Agrarian Society and Politics under the Eighteenth-Century Maratha Svarājya*. (University of Cambridge Oriental Publications, number 36.) New York: Cambridge University Press. 1986. Pp. xviii, 417. \$49.50.

In his impressive first monograph, Andre Wink makes extravagant promises and, in the occasionally turgid course of the most notable book on the precolonial Marathas yet written outside India, delivers on most of them. With an exacting command of Marathi manuscript sources and secondary works plus relevant Persian, Arabic, and Sanskrit documents as well, Wink develops a sophisticated and ultimately convincing portrayal of the rise, expansion, maturation, and decline of a uniquely Indian political entity, based in seventeenth-century western India but reaching deeply into the Deccan, Hindustan, Gujarat, and even Bengal by the 1770s.

He calls this entity *svarājya*—literally "self-rule" but

in this case much more than that—a term more apt than the conventional term “confederacy.” Wink deliberately keeps the term flexible because in the sources it appears in contrast with *pararājya*, “rule by the enemy,” and conforms to the Kautilyan notion of the Circle of Kings. Small wonder that British proconsuls and Western historians steeped in the traditions of Max Weber and Henry Maine stumbled into puzzlement when approaching the Maratha success story. Theirs was the farthest from a centralized empire that a political system can be; it studiously avoided claims to universal rule but arose and was maintained by various forms of institutionalized conflict, referred to in Marathi sources—borrowing from Arabic and Persian usage in the Islamic world generally—as *fitna*. Wink carefully defines and contextualizes this concept but then almost overuses it as a heuristic mantra. Sedition, insurrection, revolt, deviance, civil war—these are some of its connotations from a centrist viewpoint, condemning any disruption of an ordered regime. But Wink looks at this “contention/conflict” syndrome another way: “*fitna* was the normal political mechanism of state-formation or annexation, and, as it were, the negative basis of universal dominion . . . a mixture of coercion and conciliation. . . . [It] implied intervention in and making use of existing local conflicts. . . . In India, as in all Islamic states, sovereignty was primarily a matter of allegiances; the state organized itself around conflict and remained essentially *open-ended* instead of becoming territorially circumscribed” (pp. 26–27). Put another way, the Marathas built up their rule by incorporating disorder, or at least adapting to the complementarity of order and disorder; they became managers of local and regional conflict, succeeding partly because they wisely avoided claiming the universal sovereignty still owned by an ever-weaker Mughal emperor in Delhi.

The Maratha polity was one of practical, relatively inexpensive, often partly coerced sharing, not one of domination over a territory; the ruler was the one who could make revenue-free grants of land and guarantee hereditary vested rights and exemptions, not necessarily the singular authority who could “settle” the land or whose rights over all of the surplus were unquestioned throughout the realm. Indeed, by sanctioning land rights and hereditary titles, by repartitioning the proceeds of revenue, the king (or deputy) was able to uphold the dynamic tension of rival interests, to check and manage the continuing process of *fitna* itself. Thus does Wink succeed in extricating us from the centrist terminology of empire and imperial decline, by showing how the Maratha gentrification created by the Mughal empire—involving accumulations of wealth, patrimony, administrative experience, and hereditary rights—was not only part of the process of Mughal decline but also part of initial Mughal success and expansion.

In a book this ambitious, some mistaken and awkward statements are bound to crop up. Wink credits the Greeks with bringing down the Mauryan empire

(p. 21); the Mughals, in an expensive show of brute force, “immortared thousands of heads in pillars along the roadsides” (p. 197, paraphrasing, alas, Vincent Smith). His confessional labels for political or historiographical entities—“Muslim sources,” “Muslim empires,” or “Muslim domination in India”—occur in writings a book like this aims to revise and belie his claim to have read Marshall Hodgson closely. But this is an important work, accessible with effort (the glossary is six pages long), a standard for future study not only of the Marathas but of the late precolonial period generally.

RICHARD B. BARNETT
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DIPESH CHAKRABARTY. *Rethinking Working-Class History: Bengal, 1890–1940*. Princeton: Princeton University Press. 1989. Pp. xix, 245. \$35.00.

One of the weakest areas in the historiography of modern South Asia has been the history of the urban working class. Dipesh Chakrabarty has now made a signal and valuable contribution to the history of the Calcutta jute workers and their industry and has given important guidelines for more searching study of all groups in the working class of India.

Chakrabarty is associated with the “subaltern” group of Indian historians, and several long sections of this book have previously appeared in volumes titled *Subaltern Studies II* and *Subaltern Studies III* (1983 and 1984). He also contributed a defense of the whole series, “Invitation to a Dialogue,” to *Subaltern Studies IV* (1985). This enterprise, led by Ranajit Guha, takes its inspiration from Antonio Gramsci and focuses primarily on investigations of the lowest orders and most marginal groups in Indian society. It also analyzes their relations to “hegemonic” or dominant groups.

Many of the subaltern historians are critical of orthodox Marxism, although their works are offshoots of Marxist historiography, and they have been the subject of sharp criticisms from other historians of the Left, Right, and Center in India and the West. Chakrabarty frequently demonstrates the shortcomings of “Marxist constructions of working-class history” (p. 218) and puts forth as a central thesis “that a theoretical understanding of the working class needs to go beyond the ‘political-economic’ and incorporate the ‘cultural’” (p. 65).

Chakrabarty skillfully traces the rise and successes of the Calcutta jute industry from the late nineteenth century to its rapid decline in the late 1920s and 1930s. Then he shifts his focus from the owners and managers to the workers. He shows the great gaps in the knowledge that one can gather about the workers as compared, for example, with similar groups in Great Britain. He relates these gaps or “silences” to the different needs for information in India and Britain. All that the owners wanted—and the government of India that supported them—was a large labor pool, and they had

little desire to know more in detail about the lives of these men or to provide any services for them.

Chakrabarty then turns to an analysis of the recruitment patterns and living conditions of the workers. In this process, he points to the role of the *sarder*, or labor supplier, who recruited workers and then played a crucial part in controlling them. He writes, "The *sarder's* mode of operation had certain crucial precapitalist elements. He usually recruited on the basis of the often overlapping networks of community, village, and kin" (p. 112). These links, including those of religion, were basic, too, to the *sarder's* control of the workers. The author argues throughout that these "precapitalist" institutions were adapted to the needs of industrialization in India, which developed differently in India than in the West.

From recruitment and living conditions, Chakrabarty turns to three themes: the paradox of organization; protest and authority; and class and community. He moves freely through the half-century between 1890 and 1940 gathering evidence to support his general points about these themes. Although this method of presentation weakens the picture of how developments changed through this period of increasing Indian nationalist activity, the author argues his points convincingly. Again he wants to show the relation of the economic-political and the cultural and the presence of precapitalist elements in the capitalist era.

In "The Paradox of Organization," Chakrabarty analyzes the weakness of the jute workers unions and their insistence on having middle-class outsiders rather than workers as leaders. This latter pattern is also linked to precapitalist traditions in Indian society. In his other thematic chapters, he tries to account for the incidents of powerful protest—occasionally violent—that took place at the same time that organizations of workers were quite weak. He also explores the fragile class consciousness of jute workers, which was growing at the same time that communal (Hindu and Muslim community) ties and hostilities were powerful. I have reservations about the way these precapitalist traditions are described and invoked. These do not detract significantly, however, from my enthusiasm for this excellent and pioneering study that signifies a giant step forward for analysis of the Indian working class.

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S. J. M. EPSTEIN. *The Earthy Soil: Bombay Peasants and the Indian Nationalist Movement, 1919–1947*. (Oxford University South Asian Studies.) New York: Oxford University Press. 1988. Pp. xi, 161.

Commercial agriculture produced a class of farmers who were the secret to success for the Indian National Congress in rural Bombay Presidency. This small book begins by showing how regions differed in the pace of agrarian commercialization. Where commercial agriculture advanced, village power relations were trans-

formed. Cash-crop farmers were freed from field labor and from the power of professional moneylenders, village officers, and landed potentates; they became literate, their sons entered professions, and their fortunes depended on the profitability of market transactions. Where poor soil and inadequate water inhibited commercial farming, especially in the Deccan, the threat of famine lingered, and the power of professional moneylenders, hereditary village officers, and big landowners remained secure. Patrons and clients remained the central features of agrarian life. Bombay Presidency's commercial farmers supported Congress agitation and electoral victories, for the Congress party focused on issues that divided prosperous farmers from the administration, above all, taxation. From Mahatma Gandhi's rise in 1919 until independence in 1947, the Congress party won support from commercial farmers through its emphasis on no rent campaigns and tax policy. Where commercial farms were less numerous, above all in the Deccan, the Congress party was proportionately weak and relatively inactive during this period. Weak support for Congress party agitation implied more pervasive loyalty to the colonial government among village bosses.

Three general arguments about Indian nationalism emerge from this book. Nationalist politics were not merely the accumulation of alliances between regional and local leaders; there was a mass popular base for the Congress party in the countryside. Mobilizing its agrarian constituency shaped a Congress ideology that originated with the English-educated urban elite but became rooted in the "earthy soil" of commercial farms. This ideology had long-term consequences for Congress party politics, development policy, and Indian finance after independence.

These arguments are neither new nor pursued very far. The book must have been written fifteen years ago. S. J. M. Epstein addresses none of the issues or literature that have become central in Indian historical studies since 1975. His analytic framework and descriptive language are even more archaic. "Progress" and "backwardness" are used to describe positions in commercial and national development: modernization teleology undermines the book from start to finish. The Indian National Congress is identified completely with Indian nationalism and nationhood. We see not a glimmer of competing political forces inside the nationalist movement. Huge caste blocks are taken uncritically to represent self-evident components of the Indian population, and they are identified with progress to the degree that they follow the Congress party. The class relations and political imperatives of agrarian capitalists in village society disappear. Power struggles that involved landed and landless laboring classes and castes, tribal peoples, migrant labor, and commercial brokers, among many other groups, appear to have played no role whatever in nationalist politics or commercial farming. Even the commercial farmers so central to the book remain an abstraction: none actually speak in the book; their identity is merely recon-

structed from the words of Congress leaders and colonial administrators.

DAVID LUDDEN
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DAVID GILMARTIN. *Empire and Islam: Punjab and the Making of Pakistan*. (Comparative Studies on Muslim Societies, number 7.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xii, 258. \$32.00.

In a discussion in a recent issue of the *American Historical Review* (June 1989) on the current state of historiography, one writer made the point that, although the past illuminates the present, we also construct the past in the light of the present. This way of reading the past is, of course, a hazardous undertaking, but so is the opposite tendency to see the present as the creature of the past and, even more so, to pretend that one is studying the past without taking into account one's knowledge of the present. Recognition of these tendencies is especially necessary in the study of India and Pakistan, where an understanding of the nature of British rule and the process by which the British imperial structure, the Indian empire, was divided in 1947 into the two sovereign nations of India and Pakistan is an essential ingredient in the continuing political life of both countries. For India, what is most directly involved is the relationship between the two great religious communities, the Hindus and the Muslims. Although this relationship is obviously of enormous importance in Indian domestic politics, the significance for Pakistan of the historical process is perhaps even greater, for it relates to fundamental questions of personal identity, of future directions for public policy, and of national unification. David Gilmartin's singular achievement in this well-written and carefully researched study is to provide fresh insights into the process by which Pakistan became a nation.

As Gilmartin points out, the movement for Pakistan was not the only attempt in the twentieth century to create a modern Islamic state from the legacy of imperialism, but it was the first and probably the most successful. His study of the interaction of British imperial rule with indigenous social forces is a remarkable demonstration that a historian can use the present to illuminate the past without distorting the historical process. At the same time, Gilmartin's work, although he does not labor the point, is a reminder that a study of the periphery, in both a cultural and a territorial sense, can give new meaning to the history of the center. Many studies of the Pakistan movement are concerned with its ideological roots, with an emphasis on the role of the Islamic intellectual tradition in defining religious and political structures as conterminous aspects of human existence. With less theological content, this idea found expression in the "two nation theory" that Hindus and Muslims constituted not just two religions but two quite separate nations. This notion was popularized by Mohammed Ali Jinnah, but

it was also accepted in a modified form by influential Hindu historians, such as R. C. Majumdar. Many Indian nationalist historians have tended, however, to accept some version of the conspiracy theory of history that the Pakistan movement was a deliberate creation of the British to provoke strife between the adherents of the two religions in order to prolong Britain's tenure of power. Gilmartin recognizes the influence of these beliefs but looks for clues to the process of the movement in the historical experience of the territory that now actually constitutes Pakistan. Oddly enough, not many scholars have used this approach, and Gilmartin's work finds a place with the two other first-rate studies that ask questions about social structures in regions that now are central to Pakistan but have not received as much attention as other areas, such as Bengal and the United Provinces, in studies of Indian nationalism. One of these works is Imran Ali's *Punjab under Imperialism, 1885-1947*, and the other is Stephen Rittenberg's *Ethnicity, Nationalism, and the Pakhtuns*, both published in 1988.

Gilmartin's thesis is that the key to the development of the political forces that resulted in the creation of Pakistan is found in the interaction of the administrative structures of British rule with conceptions of community and state among Indian Muslims. The focus of his study is the Punjab, the vast administrative unit that came into existence after the conquest of the Sikh kingdoms in the 1840s, where Muslims made up 56 percent of the population. His argument is that here the British created a political system based on the Islamic community, with its traditional structures of patronage and hierarchy. The British could not, however, provide an Islamic center for this system, and, by the beginning of the twentieth century, with the emergence of electoral politics, the rural-urban divide made for new tensions. The first half of the book illustrates this argument through a careful examination of how the Punjab system worked. The rest of the book deals with the period after 1936 when Punjab politics were dominated by the Unionist party, which claimed to rule in the interests of the "agricultural classes," that mythic entity of the ideology of the British. The elections of 1946, in Gilmartin's reading, rejected this old colonial system, and the Muslim League, under Jinnah's powerful leadership, brought the Punjab into the political movements that were to destroy British rule. Gilmartin concludes that the tensions and ambiguities inherent in the culture and society of the old Punjab, compounded of the legacies of imperial patterns of power and Islamic definitions of state and community, are reflected in the history of Pakistan since 1947. That Gilmartin's thesis will undoubtedly come in for much criticism in both India and Pakistan is an indication of the importance of its contribution.

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PARTHA CHATTERJEE. *Nationalist Thought and the Colonial World: A Derivative Discourse?* London: Zed for the United Nations University. 1986. Pp. viii, 181. Cloth \$29.95, paper \$9.95.

The title of this book is misleading, for it deals mainly with India. The "colonial world" actually refers to the postcolonial world, and "nationalist thought" reflects the minds of those theorists who set the scene for Partha Chatterjee's thesis. His principal argument is that the postcolonial nations have generally yielded to the universal directive of reason powered by capitalism. The traditions and the insights of their ancient cultures have been almost smothered and have contributed little to the construction of new nation-state systems. Their leaders have been persuaded that modernization is indispensable whether via capitalism or state socialism.

The two opening chapters introduce the theories that Chatterjee believes have contributed to the Western discourse about nationalism. He alludes to John Plamenatz, Hans Kohn, the conservative Elie Kedourie, and Marxists such as Antonio Gramsci, Anouar Abdel-Malek (influenced by Jean-Paul Sartre) and, of course, Karl Marx. Chatterjee provides little evidence that these thinkers, Marx excepted, have influenced the growth of nations in Asia and Africa. He presents what he calls "thematic" and "problematic" characteristics that have encouraged Third World acceptance of Western ideas and institutions. The institutions, he argues, have failed to promote equality and prosperity.

In the next three chapters, the author turns to Indian thinkers. Bankimchandra Chattopadhyay, a nineteenth-century figure, exemplified Indians' respect for Western science and economics, but he criticized the West's lack of understanding of India's rich civilizations. In contrast, Mahatma Gandhi rejected the Western heritage as a base for an Indian nation. He sought a state, founded on the moral truths of the *Bhagavad Gita*, that would emerge from the villages, grow to a national movement, and expel the British without violence.

Gandhi's inspiration was seen by Jawaharlal Nehru, and others in the Indian National Congress, as a means to end colonial oppression, but Nehru felt that Gandhi's ideas were too reactionary to apply in shaping the new state. Intellectually part Westerner, Nehru envisaged a sovereign nation, directed by a revolutionary elite able to control interest groups and minorities, use science, develop the economy, and establish social justice.

Independence and Nehru's triumph created, predictably in the author's view, an unequal India oppressed by capitalism. States such as this, he warns, are threatened by ethnic disunity, separatism, and dictatorship. He foresees, however, further struggles to subvert and vanquish Western science-cum-capitalism—the source of all Asian and African troubles—and establish a new universality, a nationalist discourse reflecting ancient knowledge and native creativity.

Chatterjee almost ignores the cultural distinctions

between different non-Western societies and between their various colonial experiences, and he fails to see that their Western contacts have been positive as well as negative. And he seems to have overlooked the nationalist roles of B. G. Tilak, Lajpat Rai, and others in India. As a philosophical exercise set in history, this book seriously undervalues the evidence of history.

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RAYMOND EVANS. *The Red Flag Riots: A Study of Intolerance*. New York: University of Queensland Press; distributed by ISBS, Portland, Oreg. 1988. Pp. xv, 252. \$24.95.

This volume by Raymond Evans is about a footnote in Australian history, an upheaval that occurred almost exactly at the same time as the Punjab disturbances of 1919. Both crises had in them elements of repression and anti-Red hysteria. These riots, which have been characterized as the most serious ever in Queensland's history, exhibit an extraordinary backlash against certain groups. A small civil liberties protest induced a reaction in which empire loyalists, demobbed soldiers, the conservative press, state police, law courts, prisons, and deportation vessels were all employed against a group of Russian and Australian radicals. Three nights of violent rioting and several weeks of showcase trials manifested a phenomenon of organized intolerance. The actual disturbance was centered in Brisbane, although its ramifications were felt throughout Queensland in antiradical and anti-alien propaganda.

The tumult of 1919 leads the author to question the traditional view of Brisbane as a tranquil and drowsy town. He suggests that the Red Flag riots were not simply an isolated movement but an echo of global currents and a continuation of local conflict between race and class in Australian society. The author holds that the Russians in Queensland reflected the failed revolution of 1905. The other thorn in capitalism's side was the Brisbane Industrial Workers of the World. Mounting xenophobia and antirevolutionary sentiment targeted both these groups for reprisal. After several weeks the matter faded from public notice.

Evidently there was a considerable section of the working class, particularly the unskilled and unemployed, who were very discontented. But the overwhelming mass of Queensland workers were not in a revolutionary mood. Thus, the radical thrust of 1918–19 was easily contained. The eight interned Russians were deported; some of them were liquidated in the Stalinist purges of the 1930s. Still, the militants were never entirely silenced. By 1919 details of the Third International had begun to spread throughout Australia. The dominant ideology in Queensland today, however, continues to the Right.

This monograph is well documented, and the style is clear and forthright. It deserves better than the paper-

back production of the University of Queensland Press.

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ERIK OLSEN. *The Red Feds: Revolutionary Industrial Unionism and the New Zealand Federation of Labour, 1908–14*. New York: Oxford University Press. 1988. Pp. xiv, 296. \$59.00.

The principal theme of Erik Olsen's meticulously researched study is New Zealand's political development in the years 1908–13, when New Zealanders moved away from the paternalist liberalism of Richard Seddon, premier from 1893 to 1906, toward the emergence of political socialism and an independent Labour party. Such a sea change was, of course, part of a wider trend throughout the English-speaking world in the decade before the First World War. The agitation of the Fabians and the social surveys of Charles Booth, Seebohm Rowntree, and Mrs. Pember Reeves gave the middle classes a "consciousness of sin" about glittering wealth amid poverty and squalor. In 1912 and 1913 a wave of strikes by the "triple alliance" of miners and railway and transport workers greatly enhanced working-class consciousness in Britain. Olsen establishes the ways by which this new class feeling arose in New Zealand, aided by the recruiting drives of visiting British socialists Tom Mann and Ben Tillett and the many Australians who served as ideologues, union organizers, and newspaper editors—Robert Semple, R. S. Ross, Paddy Webb, Joe Savage, and Harry Holland among them. In fact, the Australian dimension to New Zealand's labor history, although recognized before, receives definitive treatment in this study. Naturally, the "Australasians" were not unaffected by their experiences in New Zealand. Olsen also draws attention to the important contribution of several North American Wobblies (members of the Industrial Workers of the World) to the ideology and leadership of the New Zealand Federation of Labour, or the "Red Feds" from 1910. "In the Old and the New World these leaders preached class war, direct action, industrial unionism, and One Big Union as the means for achieving a socialist millennium" (p. xiv). Their influence was especially notable in Auckland, which, with its frontier ethos and high rates of immigration, was the Chicago of New Zealand.

At the level of praxis, Olsen chronicles the rise of mass, unskilled unions among the miners, wharfies, shearers, laborers, flaxworkers, and seamen and their initiation into industrial militancy through strike action. His detailed story begins with the Blackball Strike of 1908 and finishes with the Great Strike of 1913 that first polarized New Zealand politics on class lines. The panic and alarms of Prime Minister Massey and his "Cossacks," as the special constables were called, parallel the harsh measures of Generals Louis Botha and Jan Smuts in dealing with the Transvaal miners' strike of 1913, although without the bloodshed and deporting

of union leaders. Olsen sums up the general effects of the strike: "By shifting the terms of debate within the unions to the left, and playing a decisive role in organizing the unskilled, the Red Feds moved working-class politics and organization on to a new axis. By 1911 all union leaders were socialists by self-confession, they detested Massey and distrusted the Liberals, and they recognized the central importance of organizing the industries in which their unions existed" (p. 223).

Olsen's study is a dense, closely argued work, perhaps a trifle old-fashioned in its commitment to empiricist methodology and institutional history. The narrative is thoroughly immersed in the local subcultures of work and class and assumes familiarity with the main outlines of New Zealand history and historiography. A nonspecialist would be advised to begin with a more general treatment, such as Keith Sinclair's *History of New Zealand* (1969). Although "class" receives its due, "gender" is a neglected theme, which is surprising in the light of much recent work in this area. Olsen also omits any serious consideration of the relationship between religion and popular culture, an issue that receives fuller treatment in his warm biography of John A. Lee. A small point but a pity is the separation of informative captions from the illustrations and the absence of a map. Nonetheless, Olsen's vigorous narrative breaks new ground in analyzing the urban context of class and work in New Zealand, in exploring the impact of inflation, the socialist press, and "movies" on popular mentality, and in emphasizing the theme of immigration, especially trans-Tasman and trans-Pacific mobility. Labor historians everywhere will welcome it as the first scholarly treatment of the Red Feds.

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HOWARD I. KUSHNER. *Self-Destruction in the Promised Land: A Psychocultural Biology of American Suicide*. New Brunswick, N.J.: Rutgers University Press. 1989. Pp. xvii, 284. \$24.95.

As the subtitle of this complex book suggests, this is not a traditional study of the history of suicide in America. Rather, Howard I. Kushner employs history, and the historical understanding of suicide, to resolve a central question: why, given similar social and economic conditions, does one individual choose to commit suicide while another does not? The answer, Kushner theorizes, lies beyond the conventional explanations provided by leading twentieth-century theorists. According to the author, only an interdisciplinary approach that combines the best of Sigmund Freud, Emile Durkheim, and the recent discoveries of neuroscience can explain the behavior of suicidal individuals.

Kushner uses history to inform his work in two distinct ways. First, in tracing the evolution of beliefs about suicide and attitudes toward the suicidal individual, the author describes the evolution of professional

authority over self-murder from the clergy through insane asylum superintendents to the emergence of three distinct specialties: sociology, psychoanalysis, and neuropsychiatry. The resulting competition for expertise, he argues, then limited the ability of professionals to develop a coherent theory of suicidal behavior. Through the use of aggregate statistics, sociologists explained suicide only as the result of social disintegration and the failure of appropriate institutions. Through discoveries of various brain functions, neurologists pointed to the key role played by specific neurological malfunctions. And, through case study, psychoanalysts explained suicide as the inevitable outcome of individual psychological conflict created by incomplete mourning and childhood loss.

Finding limitations in each of these methods, Kushner attempts to combine the approaches. He argues that the prime candidates for suicide are individuals who have experienced early childhood loss and lack the institutional support to resolve their psychological conflicts. Moreover, their suicidal behavior may be influenced by their biological make-up. From their diet to their faulty memory of the past, they may experience chemical changes to the brain that lead to depression and suicidal actions.

Kushner then employs history in a second way, that is, to test the validity of his interdisciplinary theory. He explores two individual case studies from the past, the suicide of Meriwether Lewis and the threatened suicide of Abraham Lincoln. Both of these men, he contends, experienced extreme loss and depression. Yet, of the two, only Lincoln was able to work out strategies to resolve despair. Lewis, on the other hand, resorted to a wide variety of suicidal behavior that failed to resolve his inner conflicts and ultimately led him to take his own life. Similarly, Kushner points to two examples of what he classifies as mass suicide—Jamestown, Virginia, in the seventeenth century and Jonestown, Guyana, in the twentieth century—to explain how the combination of individual loss, social disintegration, and restricted diet led to society-wide suicidal behavior.

For those who view psychohistory with skepticism, these test cases may pose some difficulties. To explain why an individual in the past has committed suicide is certainly demanding; to analyze an individual's failure to act may be even more troublesome. Moreover, Kushner's attempt to combine numerous fields of study often leads to slighting issues that could have been developed in greater detail. Although not a history of attitudes toward suicide, it attempts to cover these ideas in three chapters. Not a biomedical study of the field, it briefly covers different physiological approaches toward suicidal behavior.

Yet Kushner clearly has identified important issues for historians, sociologists, and psychologists. Placing suicide in an historical perspective causes him to challenge the notion of a teen-age suicide epidemic; focusing on suicide attempts and behavior, rather than completed acts, allows him to attack gender-linked notions of suicidal behavior. And, above all, his chal-

lenge to scholars to understand suicide as an individual behavior shaped by both history and culture cannot be denied. For this, his book, and the interdisciplinary approach he develops, should be considered by a wide number of scholars.

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FRANK TROMMLER and JOSEPH MCVEIGH, editors. *America and the Germans: An Assessment of a Three-Hundred-Year History*. Volume 1, *Immigration, Language, Ethnicity*. Philadelphia: University of Pennsylvania Press. 1985. Pp. xxxii, 376. \$19.95.

America and the Germans is a two-volume compilation of the forty-nine essays that were presented at the University of Pennsylvania in 1983 as part of a conference celebrating the three hundredth anniversary of the arrival of settlers in Germantown from Krefeld. Volume 1 contains twenty-four of those pieces, divided among separate sections on immigration, the "Pennsylvania Germans," ethnicity and politics, the German language, and German-American literature. Written by historians, linguists, students of literature, and other academics, from both the United States and Germany, the essays constitute a useful compendium of familiar and fresh scholarship on those topics.

For the most part, the three articles on immigration echo points that will be familiar to researchers in that field, but each highlights a specific theme. Marianne Wockeck argues that a group of German merchants was specializing in the immigrant traffic by the mid-eighteenth century and emphasizes how their innovative use of credit increased the volume of traffic. In contrast, Günter Moltmann notes that the disappearance of the redemption system by the 1820s probably reduced immigration by the extremely poor. Agnes Bretting argues that German state regulation of the immigrant traffic of the nineteenth century, which was intended in part to discourage emigration, had the opposite effect to the extent that it gave prospective migrants the promise of greater safety and security from abuse.

Five articles address the Pennsylvania Germans. Don Yoder's division of approaches to assimilation into "Americanizing," "Germanizing," and "dialectizing" follows a well-worn path. More interesting is his argument that the diversity of origins, timing, and destinations in the story of German immigration means that "there are no 'German-Americans' as such" (p. 41). Stephanie Grauman Wolf notes that many of the original settlers from Krefeld were actually second-generation Dutch refugees living in that German town. Her broader argument calls attention to the popular attribution of German identity by scholars seeking to find it and the neglect of evidence of absorption of broader colonial cultural patterns. Herman Wellenreuther explains how Pennsylvania's Germans adapted

to British dominance by stressing the importance of German influence, including the Hanoverian dynasty, on the English.

In separate essays, John A. Hostetler and Leo Schelbert reexamine the "plain people" or "Pennsylvania Dutch." Both see shortcomings in the traditional academic identification of those groups as "sects." Hostetler writes instead of the importance of the "redemptive community," which stands midway between the individual and the bureaucracies of modern life. Arguing along similar lines, Schelbert insists that the seemingly "hairsplitting" arguments in which the various groups participated were an important mechanism by which the plain people rejected the ecumenism of the Pietists and maintained their separation from an evil world.

Historians will find some of the volume's most provocative discussion in the six articles on ethnicity and politics. Resisting a point of view encountered in several of the earlier essays, Kathleen Neils Conzen argues that those who question the existence of German ethnicity in America are looking for a level of cultural uniformity that few groups achieve. Most of her rich article concentrates on the efforts of German-Americans of the nineteenth century to define that ethnicity and on the resistance of most of them to having German culture be secondary to Anglo-American.

After an essay in which Willi Paul Adams stresses the importance of ethnic leaders, Hans L. Trefousse reports the limitations that literate and liberal German-Americans such as Carl Schurz placed on the general enthusiasm within their ethnic group for the unification of Germany and the accomplishments of the new Reich. Frederick C. Luebke indirectly reinforces Trefousse's point by demonstrating how wealthy, educated subjects of the Kaiser resident in Brazil promoted a sense of cultural superiority among Teuto-Brazilians that far exceeded German-American chauvinism and that was met, during World War I, with a repression far worse than that carried out in the United States.

Switching the focus from leaders to masses, essays by Carol Poore and Hartmut Keil address the status of working-class German-Americans, especially those leaning to the Left. Poore recounts how, in 1876, German-American socialists turned the Centennial of Independence into a form of alternative cultural expression, teaching participants how far the United States had strayed from the revolutionary vision of the Founding Fathers. In his report of findings from the Chicago Projekt of the Amerika Institut of the University of Munich, Keil discusses the integration of German working-class culture into an American working-class culture. Although Keil does discuss the circumstances under which this occurred, he unfortunately does not provide specific information about the content of those cultures, which differed "in significant ways from the norms and values of the hegemonic society" (p. 203).

Essays by Jürgen Eichhoff and Marion L. Huffines are pitted against another by Joshua A. Fishman in the

section on German language. Eichhoff recounts the relatively quick demise of German, except among groups for whom the language had religious significance. He attributes the decline to several factors, including the religious, class, and geographic divisions among Germans. Huffines also emphasizes internal cultural divisions as reasons for the loss of the language, which, she notes, was well under way before the anti-German hysteria of World War I. Fishman, a well-known student of language loyalty issues, points out that the number of Americans claiming German as a mother tongue almost doubled between 1960 and 1970, which he attributes to increased willingness among ethnics to claim their heritages. Fishman admits that the actual use of German seems low but argues for the symbolic importance of the development. John A. McCarthy's essay on the teaching of German today does not have a historical focus.

The final section of the book treats German-American literature. Harold Jantz examines idiosyncrasies in the reputations of German-American writers across the centuries. Christoph E. Schweitzer calls attention to neglected early German-American authors and the need for making their works accessible. Alexander Ritter also decries the lack of attention to German-American literature, including that of modern times, and analyzes the special problems of those who write in minority languages. Martha Kaarsberg Wallach examines with insight the works of three German-American women writers of the mid-nineteenth century—Therese Robinson, Mathilde Anneke, and Fernande Richter.

Finally, German-American newspapers receive a modest amount of attention. Patricia Herminghouse engagingly describes the work of Heinrich Bornstein, an Austrian entrepreneur, in using serialized novels, with anti-Catholic themes, to attract readers to his St. Louis newspaper. Maria Wagner argues that German correspondents for *Augsburger Allgemeine Zeitung* reflected the attitudes of contemporary German settlers in the nineteenth century: early ones expressed admiration for the United States, and later ones called attention to the active antislavery movement.

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EUGENE YAZKOV *et al.*, editors. *The U.S. Two-Party System: Past and Present; A View by Soviet Historians*. Translated by SERGEI SOSSINSKY. Moscow: Progress. 1988. Pp. 414. 2 r. 50 k.

"The Marxist approach" to the study of political behavior, warn the Soviet writers of this history of the American two-party system, written in the era of Mikhail Gorbachev's *glasnost*, "has nothing in common with vulgar economic determinism." On the contrary, it is a "complicated, multifaceted dialectic process" (p. 390). Development and change, they point out, do

not take place according to some "automatism" or "rigid unilateral determinism" but result from "specific actions by people" (p. 390). History, they seem to be saying, is made by people rather than by impersonal, inflexible social forces. In probing the relationship between class status and political behavior, which they regard as fundamental to their study, "objective conditions," political awareness, "actual" social position, individual perceptions of that position, and the "political conclusions" that are drawn on the basis of these perceptions must all be taken into account. When applied to America's two-party system, the task becomes especially difficult, for the "social and class structure of capitalist society is a complex phenomenon" (pp. 391-92) that resists simple analysis. This work is a collective effort by Soviet specialists in American history at Moscow University and the institutes of the USSR Academy of Sciences, directed by an editorial board of which Eugene Yazkov is chief editor (as well as one of the contributors). The authors, although declaring at the outset that their study reflects the "Marxist concept of the two-party system," disclaim any intention of imposing their conclusions on anyone. "Ideological struggle," they insist, "does not preclude, on the contrary it implies, academic debates, exchanges of opinion and polemic" (p. 5).

The brief space of a review does not allow the debate the authors hope to encourage. Suffice it to say that American historians will find in the work's seventeen densely packed chapters (plus introduction and conclusion) much that will seem familiar and conventional, as well as provocative insights that spring from differing perspectives, with few factual slips and a clear, readable, and even idiomatic style (a tribute to the translator).

The American party system, the authors maintain, although not unique, bears certain special characteristics, the most important being that no other party "except a bourgeois one" (p. 6) has ever been able to fit into it, a circumstance that has deprived voters of a real choice of political alternatives. The system's development, from its origins in the early republic to the 1980s, has depended on changing social and economic conditions, including the class struggle. Three periods of party realignment are identified, each linked to a major turning point in the country's capitalistic development: the Era of Good Feeling, the Civil War and Reconstruction, and the New Deal. One further move toward realignment in the late nineteenth and early twentieth centuries proved abortive when the radical social movements of the Populist party and the Anti-Imperialist League were crushed. The "strong ideological influence" of the great October socialist revolution in Russia, the growing political consciousness of the working class, and the organization of the Communist party of America forced the "ruling circles" to a tighter consolidation of the "class domination of Big Business" (p. 224), thus stifling a promising opportunity to offer the voters a truly revolutionary program for the transformation of society. Although the authors describe

cyclical swings within the parties between conservatism and reformism, they find continuity in the fulfillment of the system's principal function, that is, to protect the interests of the ruling class and to safeguard the social and political foundations of capitalism.

What of the party system today and in the future? In their analysis of the contemporary state of political parties, in a "time of political disillusionment" (p. 374), the writers express impatience with the diagnosticians (bourgeois social scientists) who have all but pronounced the ailments of the system to be fatal. They take particular exception to the "arrogant and scornful attitude" (p. 376) with which ordinary Americans are criticized for their lack of knowledge and their inability to exercise independent political judgment. Indeed, the Soviet historians are more hopeful for the survival of the two-party system than the American pundits they quote and paraphrase, for they find the system's importance to the "ruling class's domination" (p. 398) to be undiminished. Obsessed by ideas of crisis and decline, America's political scientists have overlooked the underlying cause of the system's weakness: the "growing contradiction" between the "objective" course of history and the direction that the ruling class wishes history would take (pp. 398-99). The observation is not new; as some nineteenth-century analysts put it, the parties are out of step with the spirit of the times.

ROBERT W. JOHANNSEN
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WILLIAM LASSER. *The Limits of Judicial Power: The Supreme Court in American Politics*. Chapel Hill: University of North Carolina Press. 1988. Pp. x, 354.

One of the longest running debates in American public life has involved the question of whether or not the Supreme Court is too powerful for its and the nation's good. William Lasser believes it has not been, and he makes his case by adroitly blending constitutional history with an intelligent reading of the current struggle between proponents of judicial activism and judicial restraint. The result is a fresh but overargued book.

Both sides, Lasser observes, have started from the same flawed historical premise: the court is a weak institution. They have differed, however, in assessing the implications of that weakness. Adherents of an activist court have argued that judicial review is legitimate and necessary because the judiciary lacks both the will and the power of the purse. It is, therefore, incapable of reaping real political destruction. On the other hand, the advocates of judicial restraint, to whom Lasser directs most of his attention, have insisted that it is the weakness of the unelected court that makes judicial review so problematic.

Lasser especially challenges the conclusions of Robert G. McCloskey, whose books, *The American Supreme Court* (1960) and *The Modern Supreme Court* (1972), remain classics. According to McCloskey, the history of

the court demonstrates that its greatest success came when it worked on the margins of raging political controversies. McCloskey buttressed his argument by pointing to a number of historical examples, such as slavery, in which the court suffered what Charles Evans Hughes called "self-inflicted wounds." To McCloskey's mind the modern court (of Earl Warren and Warren Burger) fell into the same trap, relying on judicial power to deal with matters (such as prayer in the public schools) better left to the political branches.

Like McCloskey, Lasser also understands that political turmoil often has been transformed into constitutional disputes. "[T]he major crises for the U.S. Supreme Court," Lasser writes, "coincide[d] with the great crises of the political system as a whole," and, in each of these eras, "the people and not the Court must have the last word" (p. 253). But his chapter-length studies of three of the best-known periods of political assault on the court (the *Dred Scott* decision, the era of Reconstruction, and Franklin Roosevelt's court-packing scheme) reveal an institution better equipped for the political fray than McCloskey acknowledged. "Time and again," Lasser writes, "the Court has placed constitutional barriers in the path of a determined majority, and time and again it has survived and prospered" (p. 255).

According to Lasser, strength, not weakness; resilience, not inflexibility; and popular support, not suspicion have been the signal features of the court's history. "[T]he modern Court," he insists, "is not in mortal peril" (p. 249). Lasser also asserts that the justices have exercised their authority free of overt political constraints, in large measure because they have engaged in a continuing dialogue with the two other branches about the proper development of constitutional doctrine. In Lasser's view, the court's commitment to the principle of separation of powers, coupled with the frequent unwillingness of Congress to come to grips with the most controversial public issues, has made judicial review a necessary feature of the American scheme of governance.

Lasser's study holds few surprises for constitutional historians, who will find that in each instance his interpretation parallels positions long since sketched by Don Fehrenbacher on the *Dred Scott* decision, Stanley I. Kutler on Reconstruction, and William Leuchtenburg on the court-packing scheme. What Lasser does—and he does it well—is to offer these historical episodes in a way that advances the contemporary debate about the court. Not all scholars will agree with his belief in the virtues of judicial activism, and some readers are likely to grow weary of the repetitive nature of his argument. But none of them will fail to appreciate Lasser's thoughtful interdisciplinary contribution to an already gigantic body of literature.

KERMIT L. HALL
University of Florida

ALAN TRACHTENBERG. *Reading American Photographs: Images as History, Mathew Brady to Walker Evans*. New York: Hill and Wang. 1989. Pp. xxi, 326. \$25.00.

The most important of the several complex and subtle arguments that unite Alan Trachtenberg's half-dozen essays goes something like this: during the century after the first American daguerreotypes were made in 1839, photographers, private business people, and public officials who shaped the history of photography in the United States sought to make visible, through depiction of selected external realities, underlying values of American society. Most provided a misleading image of the actualities that these values helped sustain by focusing on ideals and accomplishments while ignoring, and often willfully suppressing, evidence of deficiencies in the prevailing economic and social orders. Because of inclusions and omissions dictated by obfuscating social conventions and class interests, documents such as Mathew Brady's portraits of illustrious Americans and U.S. Geological Survey studies of the West distracted attention from the violence to the natural world and other human beings that accompanied American economic development, although Trachtenberg also gives credit to these projects' revelatory aspects. A few photographers such as Timothy O'Sullivan (who worked with the Geological Survey), Lewis Hine (in his photographs of child labor), and Walker Evans transcended dominant ways of seeing. They were able to reveal society's contradictions and limitations in the medium itself and in their own beliefs. Alfred Stieglitz, another photographer of vision and unsurpassed intensity, concealed as well as revealed through his delusory attempt to "free the image from its social content" (p. 289). Among writers affected by photography were Herman Melville and Stephen Crane. Their unblinking look at war photographs helped give power to their verbal descriptions of its horrors. Walt Whitman used photographically derived prints of himself to promote democratic respect for the dignity of undervalued social classes and of bodily impulses usually left unmentioned (p. 63).

Trachtenberg develops his arguments through close analysis of individual photographs, with attention to the textual and visual context in which they appeared, combined with incisive accounts of interactions between main currents in American culture and significant developments in the art and industry of photography. For example, by interpreting images with help from the light provided by other sources, such as Karen Halttunen's research on the intense concern with personal character in the 1840s, he demonstrates that "the near-universality of the experience of sitting for one's daguerreotype circulated throughout America in a new regard for visibility, for one's own image as a medium of self-presentation" (p. 29).

The illuminating interpretive energy derived from Trachtenberg's indignation with exploitation of workers and other forms of oppression sometimes leads to readings shaped more by his desire to affirm the

humanity of abused groups, a humanity quite apparent without prompting from the author, than by the evidence. He writes of J. T. Zealy's portraits of slaves that "the eyes of the enslaved Africans can only reveal the depths of their being—for, as naked slaves, they are permitted no social persona" (p. 56). Slaves knew that hiding their ideas and emotions was essential to survival. Zealy's photographs are powerful documents and deserve all the attention that Trachtenberg gives them, but it seems equally plausible that, for instance, "Jack" (illus. no. 12) was either outraged or fascinated by the experience of being photographed but disguised his feelings in a way that made them inaccessible to the historian as well as to his owner. The complexities of public response to photographs also seem more elusive than Trachtenberg suggests. He proves that stereographic companies involved in the revival of interest in Civil War pictures in the 1880s sought to focus attention on the "thrilling" aspects of war rather than on difficult moral questions. But he offers no support for his assertion of what this revival of interest indicated about the public's reaction to photographs that pointed to the "terrible reality" of war: "the public relished the terror—or found ways to deny it" (p. 77).

Because Trachtenberg's views are strongly stated and often speculative, the book bristles with challengeable assertions. But attentive readers will come away with a new and fuller understanding of the numerous photographs that Trachtenberg discusses and with sharpened habits of seeing.

GEORGE H. ROEDER, JR.
School of the Art Institute of Chicago

KARAL ANN MARLING. *George Washington Slept Here: Colonial Revivals and American Culture, 1876–1968*. Cambridge: Harvard University Press. 1988. Pp. xii, 453. \$39.95.

In New York City a year ago this past April, a three-day celebration of the anniversary of George Washington's inauguration organized by Radio City Music Hall Productions included the second largest fireworks display in the city's history (the Statue of Liberty show holds the record), a parade, and a speech from President Bush on the continuity of the presidency "from George to George." The number of participants, audience and principals, exceeded one million.

There is no question, of course, about the historical importance of Washington or of the event commemorated. But what of the significance of such popular civic exercises in themselves? Karal Ann Marling argues that such events record the popular uses that Americans have made of their past and, therefore, reveal much about contemporary cultural values and concerns. Given the current debate about popular historical knowledge (or the lack of it), the subject is a serious one, but Marling's lively and irreverent treatment also suggests that it can be entertaining as well.

The changing popular image of Washington is alone

a challenging topic. Marling begins with Ronald Reagan's invocation, in his second inaugural address in 1985, of the legend of the besieged general praying in the snow at Valley Forge. She then flashes back to the Philadelphia World's Fair of 1876, the major public event of that centennial year, when the image of Washington as a symbol of national unity (so useful during the era of civil strife) was replaced by "a more complex figure who both endorsed a century of material progress and expressed reservations about the price paid for it" (p. vii). Over the years, Washington appears as (among other things) the engineer, the good businessman, the courtly gentleman, and the enterprising young farmer, depending on contemporary official, commercial, and popular needs. But this is more than a study of the changing images of the nation's first president. Marling also intends to demonstrate the ways in which Americans have viewed their colonial past in general, a much more diffuse subject and a more difficult task for the historian of popular culture.

The major achievement of the work is the author's ambitious attempt to be inclusive, to present evidence drawn from the full range of cultural expression, and to interweave the various threads into a coherent pattern. Marling is at her best, as one might expect, in her treatment of the visual arts. But the major strength is also the primary weakness of the book. Some of the material evidence is handled with care and considerable insight; some of it is dealt with superficially and anecdotally. Although truly interesting connections are suggested, many of the relationships are primarily impressionistic, and few historians familiar with recent scholarship will be surprised by any of the book's general conclusions.

There are several methodological and definitional problems as well. Documenting mass attitudes would seem to require more statistical evidence than is provided (how many people participated in or witnessed the activity in question; how many purchased, read, or displayed the artifact?). One can also question whether the products or activities of commercial entrepreneurs, social elites, or even the most civic-minded politicians reflect equally on the public mind. A similar question can be raised, of course, with the products of the "fine arts" and "popular arts." There is even a disturbing vagueness in the overall subject of the study. Because it would be difficult to find a single year, or even a month, in the last one hundred years in which a concern with the American past was not manifested somewhere in the culture, what exactly is meant by a "popular revival"? The term "colonial" is also used quite loosely, sometimes seeming to stand for nothing more specific than "old-time America."

Despite these difficulties, this book is rich in detail and engagingly, even provocatively, written. It should please a wide popular audience even as it does little to counter the trivialization of Washington's memory or

the colonial past in American culture that it describes.

REID BADGER
University of Alabama,
Tuscaloosa

WILLIAM H. WIGGINS, JR. *O Freedom! Afro-American Emancipation Celebrations*. Knoxville: University of Tennessee Press. 1987. Pp. xxi, 207. \$24.95.

William H. Wiggins, Jr., draws on personal interviews, his own observations as participant observer, Afro-American newspapers, and the *Congressional Record* to "explore the origin, development, and cultural significance" of African-American freedom celebrations from the era of the Civil War to the establishment of Martin Luther King, Jr.'s birthday as a national holiday. He identifies "at least fifteen" different dates that African-Americans have observed to commemorate freedom from slavery alone (p. xix). Most of these dates, such as September 22, January 1, February 1, June 19, and August 8, are associated with events that occurred during the Civil War era, including the issuance of the Emancipation Proclamation and the ratification of the Thirteenth Amendment. But several celebrations commemorate such earlier developments as the abolition of the foreign slave trade to the United States, emancipation in the West Indies, the abolition of slavery in various northern states, and instances of the manumission of certain individuals, families, and small groups.

Emancipation celebrations during and immediately following the Civil War reflected, of course, a culture expressive of slavery, especially festivities that featured such activities as music, dancing, parades, athletic games, eating and drinking, and religious worship. Later observances continued these cultural motifs, although in forms that also exhibited contemporary African-American culture. Later celebrations also incorporated references to political and social concerns inspired by the postslavery experiences of African-Americans and their continuing struggle for freedom and full citizenship rights in American society. Such observances frequently served, too, as occasions for family reunions and homecomings for black southerners who had migrated to the North and West.

Wiggins associates the freedom celebrations with a wide array of other African-American observances, ceremonies, and cultural activities, ranging from beauty pageants, contests for campus queens, fashion shows, fraternity and sorority initiations, music festivals, and sports events to the observance of Negro History Week and Black History Month. In doing so, he often implies and sometimes asserts a monistic, direct, lineal relationship between such activities and the freedom observances, with provenance being accorded to the emancipation celebrations. He ignores, therefore, other plausible lines of association, less direct and probably much more complex, for the evidence that he provides cannot sustain an assertion

beyond his general conclusion, namely, that the rites associated with the freedom observances and those found in other African-American social and cultural activities derive from common roots in African-American culture. The case for anything more than that remains to be established.

Nevertheless, through the lens of the emancipation observances, Wiggins provides both readers and scholars with much useful information demonstrative of the unique nature of the African-American experience and suggestive of fruitful lines of further inquiry that he or other researchers may wish to pursue.

ARNOLD H. TAYLOR
Howard University

TERRY G. JORDAN and MATTI KAUPS. *The American Backwoods Frontier: An Ethnic and Ecological Interpretation*. (Creating the North American Landscape.) Baltimore: Johns Hopkins University Press. 1989. Pp. xiii, 340. \$36.00.

This book purports to show that an obscure group of Savo-Karelians originally from Finland came to New Sweden about 1640 to set the agenda for conquering the continent. No other group can make this claim because no one else was culturally "preadapted" (p. 250), not the Celts nor any "poorly suited" Germanic group whether English, Dutch, or German. These particular Finns brought to North America not only log cabin techniques (as suggested by others) but almost everything for pioneering, such as dispersed settlement, axes, shifting cultivation, hunting skills, open-range livestock, worm fences, and rye whiskey. A "secondary thesis" is that native agriculturalists, "in particular the Delaware tribe," were "the next most significant contributors to the backwoods pioneer adaptive systems" (p. 36). These propositions lead Terry G. Jordan and Matti Kaups to state that the frontier expansion of the "midland" cultural region could not have happened so rapidly without the initial Finn and Delaware impetus. Their skills were taken up by the Scotch-Irish especially and others from the hardscrabble periphery of Germanic northern Europe. Germanic peoples followed. The authors reject, as key methodological "mainsprings," five orientations in cultural and historical geography and history as being "normative" or "acultural" in favor of "particularistic cultural ecology" (p. 32, 247).

They may be right about these Finns, but can we ever know? Despite making their assertions repeatedly, the authors admit that their evidence is "circumstantial" (p. 248). In fact, in looking over the long bibliography and checking references, it is obvious that their North American evidence is selective and much of it based on hearsay from secondary and antiquarian accounts. The documentary evidence is thin and uncritically accepted. Nothing is provided on the Delaware to distinguish them from other native agriculturalists and hunters. If the evidence is scant, should not caution be exercised?

Among a great number of assertions that can be questioned, let me pick out three of the most egregious. In the preface, Jordan indulges in a *mea culpa*: a Germanophile, he searched in vain for the origins of log construction in central Europe and discovered Kaups—an instant conversion. But the clarity of the conversion is sullied. Who is allowed into the Savo-Finnish inner sanctum of esoteric frontier skills? Celts certainly, but not the English. But wait. "The majority of English immigrants to the Midland hearth came from . . . lowland Britain[.] . . . Germanic core Europeans accustomed to stabilizing selection and ill suited for frontier life. Others, however, had been born in highland fringe counties of England, such as Yorkshire, Dorset, Westmorland, and Cornwall. In spite of being ethnically English, they were fringe folk very much like Celts, attuned to diversifying selection" (p. 61). At what elevation did one qualify? The authors' distinction is not ethnic after all but environmental, a methodological stance they denounce (p. 24). Or are they implying that, while Scotch-Irish and Yorkshiremen were agriculturalists at home, because they were marginal to the power structures, they were encouraged or forced for some reason to take up land clearance on the frontier after learning from the Savo-Karelians? But social relations are rejected, too (p. 29).

Second, not only the conclusions but also the facts of relevant scholars are dismissed. The distribution of ethnic and religious groups shown on maps assembled by James Lemon on Pennsylvania and Robert Mitchell on the Shenandoah are ignored. Why? Is it because these show that many English and Germans settled on the frontier? If the authors had carefully reassessed the tax lists and distributions of churches (representative of different ethnic groups) and cast reasonable doubt on these analyses, then one might be willing to engage in debate. Instead, tossed off are mainly episodic citations: "for example, the Yorkshireman living on the Susquehanna in 1705" (p. 61).

Third, another mistreatment of others' work occurs in the authors' discussion of dispersed settlement and freehold tenure. They ignore the Pennsylvania evidence that shows that overwhelmingly settlers scattered onto separate large private holdings. They dismiss the most focused assessment of settlement in early New England yet undertaken, Joseph Wood's "Village and Community in Early Colonial New England" (*Journal of Historical Geography* 8 [1982]: 333–46), by "a careful reading of the literature" (p. 125). Which literature? One need only go to the *Records of the Governor and Company of the Massachusetts Bay in New England*, edited by N. Shurtleff (vol. 1, pp. 157, 181, 257, 291), to see that the tight communal spatial system broke down quickly.

Except for the discussion on the log cabin and some other minor material items based on research, this book is without merit. Those who would make something of landscape patterns by connecting culture to society, economy, and political power will find this work inadequate. Those who are inclined to load

cultural baggage to reify this or that ethnic group without taking serious account of social interplay and timing should take warning.

JAMES LEMON
University of Toronto

HELEN C. ROUNTREE. *The Powhatan Indians of Virginia: Their Traditional Culture*. (The Civilization of the American Indian Series, number 193.) Norman: University of Oklahoma Press. 1989. Pp. viii, 221. \$18.95.

Helen C. Rountree characterizes this work as "a historical ethnography," a comprehensive study of the life-ways of the Powhatan Indians based on the historical documents left by seventeenth-century English chroniclers of Virginia. Admittedly, these observations of Indian culture are both biased and incomplete. They utterly fail, for example, to capture the subtleties of Powhatan religion and world view because the Europeans never came to understand the culture of their Indian neighbors in any depth. By the time any European was interested in such a level of understanding, Powhatan culture had been transformed by the effects of European contact, disease, warfare, and land loss.

The author's method is ethnohistorical. She analyzes the data from historical sources in light of the comparative understanding of American Indian cultures that is the legacy of more than a century of professional anthropological study. She frames her study as an attempt to interpret the Powhatan confederacy as an example of a chiefdom, concluding that its government "was neither powerful enough nor formally enough organized that we can call it a 'state' by anyone's definition" (p. 148). Contemporary English writers noted that what they called the Powhatan "empire" did not long predate the contact period, and Rountree believes that its consolidation resulted from external threats to the Algonquian-speaking peoples living along the fall line from alien tribes to the west and north and from the English to the east. She suggests that, in the absence of new documentary sources, any definite answers about the formation of the confederacy will have to come from archaeology.

The book begins with a concise presentation and evaluation of the ten authors and their writings on which it is based, from John Smith's "A Map of Virginia" (1612) to Robert Beverley's "Of the Natives" in his *The History and Present State of Virginia* (1705). This is followed by a detailed and helpful discussion of the natural environment in which the Powhatan lived. The body of the work consists of a series of chapters that synthesize information on Powhatan culture by subject, ranging from subsistence and town organization to social order, sex roles, politics, war, medicine, and religion. This systematic inventory of cultural traits, supplemented by archaeological material and fleshed out by comparisons with other Indian groups, exhaustively mines the documentary sources for data. A final chapter draws the ethnographic details together

and assesses the Powhatan confederacy as a chiefdom. The evidence suggests that, although Powhatan personally monopolized certain luxury goods such as copper, the peoples of his "empire" did not pay him tribute in foodstuffs, and, therefore, his chiefly office could not function as a redistributive center for society at large. Powhatan and his subordinate chiefs were closely tied to religious leaders, and ultimately the civil and religious hierarchies were mutually supportive and, in fact, may not have been differentiated at all.

This volume serves as a historical baseline for an understanding of the Powhatan Indians as exemplars of the woodland culture of eastern Algonquian Indians. It is a valuable synthesis of Powhatan culture as it existed at the time of early European contact and will be appreciated by historians and anthropologists alike. In a forthcoming companion volume, Rountree builds on this foundation to trace the history of the Powhatan Indians from the early seventeenth century to the present.

RAYMOND J. DEMALLIE
Indiana University,
Bloomington

JAMES H. MERRELL. *The Indians' New World: Catawbas and Their Neighbors from European Contact through the Era of Removal*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1989. Pp. xv, 381. \$32.50.

This study overlaps with and exceeds both Douglas Summers Brown's *Catawba Indians: The People of the River* (1966) and Charles M. Hudson's *Catawba Nation* (1970). Unlike those books, which cover the period from Spanish exploration to modern times, James H. Merrell's work ends in the mid-nineteenth century (unfortunately without mentioning that the modern Catawbas still possess a reservation and a vital tribal identity). But to assess this shorter timespan, Merrell has delved more deeply into both archaeology and history than either of the others attempted to do. Brown's study is a history book; Hudson's is predominantly anthropological; Merrell's is ethnohistory.

The "Catawbas" known to history were an aggregation of peoples, primarily Piedmont Siouan-speakers. So the first chapter covers all Piedmont Siouans from South Carolina to Virginia. All were poorly described by European observers before the mid-eighteenth century, and all met aggressive coastal plain aliens—Spaniards, English, Powhatans, and Muskogean-speaking Cofitachequis. The Europeans brought diseases, and the Piedmont peoples had to contend with the personal and cultural aftermath of disastrous epidemics. The two coastal plain chiefdoms faded and were replaced with competing English traders from Virginia and South Carolina. Thus, trade goods and intensive hunting to pay for them—a further, but initially welcome, disruption of Indian life—appeared.

Relations between Indians and traders soured as

traders increased and Indians became more dependent on their goods. The Yamassee War did much to make both sides more circumspect. Meanwhile, the decimated Piedmont tribes set about consolidating for safety, a necessity in the face of continued Iroquois attacks. Refreshingly, Merrell does not merely say consolidation occurred; he shows, partly through existing documents and partly through anthropological studies of the process elsewhere, how it was engineered. The original Catawbas became dominant, although other tribal identities persisted; thus, all the Piedmont Siouans who settled with them in South Carolina became known to outsiders as Catawbas.

Ironically, just as the new, multi-ethnic Catawbas gained their strength, two more catastrophes struck. English settlers moved en masse into the Piedmont in the 1750s (described in detail in chapter 5), causing angry clashes. And smallpox wiped out two-thirds of the tribes' people in 1759. That disaster, coupled with the Catawbas' service with the English in the Seven Years' War and with the Americans in the revolution, enabled the Indians to forge a new, sympathetic image as ever-peaceful allies of the South Carolinians. The image was useful for many years, camouflaging the retention of many traditional practices and a reluctance to assimilate. But hardening white attitudes in the Removal Era wiped out memories of Indian loyalty, and South Carolina persuaded the Catawbas to sell their reservation (already largely leased out) in 1840. Their internal disagreements prevented their acceptance of any plan for new lands, and they were left landless. The book ends with their return by 1850 to 630 acres in their old territory (the "new reservation") and Merrell's reconstruction of the traditions in which the next generation of children—Frank Speck's informants in the 1920s and 1930s—was reared.

The text includes broad description and interpretation but not much detailing of events that one finds in traditional histories (the very comprehensive notes make up for it). Merrell is clearly aware that in sticking too minutely to the documents—made, of course, by non-Indians—one runs the risk of telling the non-Indian side of the story or, worse, of writing a history of the non-Indians rather than the Indians. No such failing clouds the reader's view here. One finishes the book feeling almost as though one has done fieldwork among the historical Catawbas.

The main contributions of the book are three. Being thoroughly interdisciplinary, it has the maximum possible depth for the time and people covered. It has a vivid reconstruction of the Indians' experience of disastrous contacts with alien people and diseases and the nation-rebuilding that they undertook afterward. And there is a realistic emphasis throughout the book on how Indian society, rather than collapsing, adapted to changing conditions. All three of these attributes are expectable today in any good Indian history. But

Merrell succeeds in a study of eastern American Indian people, and that makes the book outstanding.

HELEN C. ROUNTREE
Old Dominion University

ALAN GALLAY. *The Formation of a Planter Elite: Jonathan Bryan and the Southern Colonial Frontier*. Athens: University of Georgia Press. 1989. Pp. xx, 282. \$35.00.

This is an intriguing and well-written biography of Jonathan Bryan (1708–88) who emerged from obscurity in the colony of South Carolina to become a leading political and economic figure in Georgia as it developed from a frontier to a plantation society. His parents migrated from England in the seventeenth century as members of the Anglican church. Early in his career he engaged in trade with Indians and the expanding rice economy and soon developed an insatiable desire for additional plantations. His knowledge of the southern terrain and his familiarity with the Creek Indians led him to assist James Oglethorpe in Indian negotiations. He met George Whitefield in 1740 and was so inspired by his preaching that he supported Whitefield's orphanage and promoted education and Christianization of slaves but never consented to emancipation. He abandoned the Anglican church in 1743 and formed an Independent Presbyterian congregation.

Bryan gradually increased both his landholdings and slaves in South Carolina and survived the economic depression of the 1740s by diversifying his efforts beyond rice to indigo and lumber. As a planter of "middling to higher rank" (p. 61), he became a member of the Commons House of South Carolina in 1746 but was not one of the colony's ruling class. His ambition for land and political power led him to move to Georgia in the early 1750s. His total accumulation of land in South Carolina and Georgia eventually reached over thirty-two thousand acres with more than 250 slaves involved in his plantation operations. He served on the council and in the upper house of the assembly of Georgia from 1755 to 1770 until he was suspended by the king of England for chairing the protest meeting on nonimportation. He was then elected in 1771 to the lower house of the assembly but was expelled two years later for unauthorized visits to Florida to negotiate with Creek Indians and the emerging Seminole tribe for still more land. Plans for his independent colony in Florida were disrupted by the American revolution. Bryan turned patriot in the revolution and served on the revolutionary council of Georgia but was captured along with one son in the fall of Savannah in 1778 and held prisoner for twenty-two months. After his release from prison, he returned to the Georgia executive council for over a year.

The author expresses "great respect for the Turnerian emphasis on the frontier as process" and "deep concern for examining the evolutionary aspects of the frontier" (p. xvi). He then argues that the "frontier experience . . . was the most important factor in the

creation of the South" and that the "frontier was seminal in the creation of the American character" (pp. xvi–xvii). Eschewing the role of the ideal of the country gentleman that was influential among many southern planters, he concludes that the paternalistic ideology of Bryan and the southern elite was the result of the transformation from frontier to plantation society. This exclusive emphasis on frontier influences is not entirely convincing, but the author is successful in demonstrating Bryan's paradoxical motives of piety and charity to all groups in society as a "social radical" along with his ambition for wealth and power. If these proved incompatible, wealth and power prevailed. The author, then, may safely conclude that Bryan with his idealism "was not typical of the majority of planters. But he was representative of the slaveholding elite" (p. xviii). There is surprisingly little information about the management of the many plantations owned by Bryan, and there is no identification of his intellectual interests, if any, beyond religion, politics, and land acquisition.

The volume includes effective maps of Bryan's landholdings, and the appendixes contain valuable information on his attendance record at Georgia council meetings, petitions for land in Georgia, land conveyances, and vote tallies of factions in the Georgia assembly of 1773.

W. STITT ROBINSON
University of Kansas

PETER A. COCLANIS. *The Shadow of a Dream: Economic Life and Death in the South Carolina Low Country, 1670–1920*. New York: Oxford University Press. 1989. Pp. ix, 370. \$39.95.

Peter A. Coclanis describes his book as "an analytical essay rather than a work of narrative history" (p. vii). The author does not give himself enough credit. Although he has not written a traditional narrative, he has told a compelling story of the tragic rise and fall of the rice economy of the South Carolina low country.

The author offers his readers three varied contributions. First, the introduction and two chapters that follow imaginatively interpret the history of Charleston's architecture between 1680 and the eve of the revolution in order to reveal the basic "pathogenic nature" (p. 11) of Charleston's economy; establish the mercantilist founding of South Carolina; and describe the ecological setting of the low country. (A discussion of relations between Europeans and American Indians appears in this section.) Second, an exceptionally ambitious set of notes (128 pages) provides not only necessary technical explanations but also the most far-ranging and thoughtful survey we have of the interdisciplinary literature pertinent to the history of economic life in South Carolina. Third, the two longest chapters, although less original and more abstract than the earlier ones, structure the history of the low country under the rice regime and provide important

refinements of our data on prices, production, trade, productivity, wealth, and population. In the process, Coclanis substantially improves the economic definition of the history of South Carolina.

Perhaps the most striking aspect of the author's approach is his unflinching belief in "the power of materialist explanations of the past" (p. viii). In his account, capitalist profit maximizing is the engine of economic decisions and of the social development of South Carolina. Such was the case, according to Coclanis, even during the formative mercantilist era and during the decline of South Carolina's rice industry. He sees no inconsistency between planters maximizing profits and at the same time accepting the "mercantilist desiderata" of "staple production, dependence, and subservience" (p. 144). While the low country's success in fulfilling mercantilist expectations through specialization in rice led to high rates of profit and large accumulation of wealth in the eighteenth century, that same success lured the planter class and, consequently, low country society into what became a disastrous blind alley in the nineteenth century. The legacy of rice was "an economic structure characterized by disarticulation, distortion, and asymmetry, a poor, uneducated black majority, a desolate landscape and a forlorn and miserable history" (p. 157).

The emphasis on the potency of the marketplace in shaping low country society is particularly welcome from someone as well versed in issues of class and race as Coclanis. He recognizes that major issues at the intersection of social and economic change remain open for debate. For example, in explaining the decline of rice, he argues that it was underway before the Civil War and was primarily a consequence of the imperial promotion of rice production in southern India and Java. But he recognizes that his methodology cannot rule out the significance of either Civil War destruction or, following the research of John Scott Strickland, the postwar resistance of former slaves to market forces. Stating the matter somewhat differently, Coclanis knows that, although his materialist model may capture the behavior of planters and investors, it is not sufficiently robust to describe the culture of propertyless people and to explain in a definitive fashion economic outcomes that depend to some uncertain degree on "non-market" actions. Thus, Coclanis has opened the door to scholarly debate at the same time that he clarifies an important set of historical issues. His book, then, represents an exemplary effort to bring cliometric analysis into the mainstream of historical discourse.

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ELIZABETH BLACKMAR. *Manhattan for Rent, 1785-1850*. Ithaca: Cornell University Press. 1989. Pp. xiii, 348. \$29.95.

New York City seldom lacks chroniclers. While Philadelphia and Boston possess their champions and Chicago and St. Louis merit their scribes, the island of Manhattan continues to excite the talents of social and urban historians. Indeed, the last few years have witnessed a steady outpouring of books that deal with the growth and character of this metropolis, especially during the period between the American revolution and the Civil War. Works by Sean Wilentz, Christine Stansell, Paul Gilje, and others have enriched our knowledge of Manhattan, and they have demonstrated what the new social history is all about in terms of class relations. Elizabeth Blackmar's study represents yet another fine addition to this ever-growing body of scholarship.

This book examines the rise and development of rental property in Manhattan during a period of dramatic changes. As such, it deals with an important yet heretofore neglected subject that has much to tell us about the uses of land, the transformation of housing, and the relations among classes. Blackmar begins by detailing how Manhattan's *rentier* class of large landowners enticed artisans and laboring folk to lease their properties during the colonial period. The promise of long leases, generally twenty-one years or more, enabled individuals of moderate means to exert control over their combined shops and dwellings, where "integrated households" of masters, journeymen, servants, apprentices, and slaves resided together. Long leases also prompted some artisans to speculate; they could build additional houses to rent to others. As for the landlords, they could pocket their ground rents while anticipating future capital gains on the property.

This situation did not last. By the nineteenth century the integrated household disappeared, victim of an emerging cash economy that treated housing as a commodity. Dwellings and workshops became separate entities, and boarding houses now lodged journeymen and laborers who lived apart from their employers. Manhattan's growing population, in turn, prompted the *rentier* class to construct more modern-style housing, dwellings that middle-class families aspired to own in order to establish themselves as independent proprietors in the republican tradition. Such housing also restructured domestic living arrangements, since dwellings became centers of family life that fostered new cultural values and responsibilities among men and women. And yet, as Blackmar makes clear, the aspirations of homeownership proved beyond the means of many because of spiraling costs. Poorer folk found themselves crowded into smaller tenements by landlords determined to maximize profits through higher rents and subdivision of dwellings. Laboring individuals, however, devised tactics to ameliorate their plight; they established their own timetables for rents, organized informal word-of-mouth boycotts of harsh landlords, or simply absconded without paying the rent. The *rentier* class continued to hold the upper hand, but working-class tenants were not without options.

Blackmar marshals an impressive argument, delving deeply into the source material and developing new insights from it. The author's description of the myriad uses of the public landscape and its different meanings to various classes sparkles as does her analysis of housing in general. There is much to applaud here. Still, one wonders if the book would have been enhanced by looking more at the role of ethnic householders and renters—an important component of the antebellum population. How German and Irish immigrants reacted to the types of housing available might have been fruitful to explore. In considering why tenants failed to organize politically against landlords, Blackmar could have considered the role of outmigration. Even poor New Yorkers could trek to the less expensive countryside of Brooklyn, New Jersey, and Long Island, returning to Manhattan during different times of the year.

These are minor criticisms, however. Blackmar has furnished us with a fine book that merits serious attention. This will probably remain the definitive social history of housing in eighteenth- and nineteenth-century Manhattan for a long time to come.

ROBERT E. CRAY, JR.
Montclair State College

SHARON V. SALINGER. *"To Serve Well and Faithfully": Labor and Indentured Servants in Pennsylvania, 1682–1800*. New York: Cambridge University Press. 1987. Pp. xiii, 192. \$29.95.

Sharon V. Salinger addresses three key questions in this well-researched and important book. First, she explores the relationship between slavery and indentured servitude as forms of labor. Second, she examines the character of indentured servitude and how it changed over time. Finally, she asks why unfree labor yielded to free labor at the end of the eighteenth century in Pennsylvania.

In the opening chapter, in an insightful discussion, Salinger contrasts the Pennsylvania experience with the development of unfree labor elsewhere in colonial America. Unlike unfree laborers in most other locations, those in Pennsylvania did not live entirely in rural areas, were not dependent on a staple crop, and did not become slaves. In Pennsylvania, slavery and indentured servitude persisted side by side with the relative number of slaves increasing only when servants became hard to obtain. This circumstance occurred most frequently during colonial wars when immigration, especially from Germany, was disrupted and when many servants escaped bondage by enlisting in the army. Servants, however, remained the labor source of choice; servants cost less to buy and keep. Moreover, unlike with slavery, there were no doubts about the morality of servitude for Quakers.

The strongest part of the book is Salinger's discussion of the changing nature of indentured servitude. In the late seventeenth century, most servants arrived in

Pennsylvania with their masters and were considered part of an extended family. After completing their term, many obtained property and achieved modest success. This almost benign form of bound labor gave way in the eighteenth century to a more exploitative system. Servants became a commodity and often were from non-English ethnic groups. Many of these servants worked in Philadelphia for artisans, but few later obtained much property. After 1750, indentured servitude declined. Most indentures were for female domestic servants or blacks who bound themselves out for a few years after their emancipation.

Salinger's analysis of the demise of indentured servitude relies on the laws of supply and demand. She argues that, when more free laborers became available, unfree labor was no longer needed. Salinger may be right, but she depends too heavily on some key unproven assumptions. For example, Salinger notes that masters in the free market of late eighteenth-century Philadelphia seldom employed the same workers consistently for the entire year. Salinger assumes that employers merely dismissed their employees when work was scarce. This does not give the laborers much credit. Perhaps the workers switched jobs in pursuit of better pay. And Salinger does not show where the new free labor force came from. The number of unskilled workers might have fluctuated with market forces, but skilled journeymen did not appear simply from demographic growth. They had to be trained.

A more careful look at the source of free laborers in artisan workshops is called for. Salinger examines immigrant indentured servants in great detail, but she does not fully consider apprenticeship, another form of indenture. Was apprenticeship, which bound laborers even longer than immigrant servitude, growing at the end of the eighteenth century? If so, did apprenticeship contribute to the decline of immigrant bound labor just as production by skilled labor was increasing?

This study, despite the questions raised above, is a first-rate piece of scholarship. In it Salinger fills in the contours of the history of indentured servitude, providing detail on the character, the numbers, and the life course of this important source of early American labor. Equally significant, Salinger is not content with sketching the outlines of the story of indentured servants; she includes crucial detail on individual lives that gives us a sense of the experience of being an immigrant indentured servant in colonial Pennsylvania.

PAUL A. GILJE
University of Oklahoma

RANDALL BALMER. *A Perfect Babel of Confusion: Dutch Religion and English Culture in the Middle Colonies*. (Religion in America Series.) New York: Oxford University Press. 1989. Pp. xi, 258. \$24.95.

A welcome addition to the scholarship exploring middle colony religion, this study traces the development of the Dutch Reformed Church from the English

conquest of New Netherland in 1664 to the revolutionary years. More than a rich denominational narrative, Randall Balmer's work documents the decline of a distinct ethnic society and its assimilation into British America. He follows the Dutch clergy and laity as they responded to the mandate of Anglicization, and he identifies class interest as a significant factor determining that response.

In beginning with the conquest, Balmer posits a growing, stable Dutch colony eclipsed by English invasion. Instead of a united Dutch response, Balmer notes mixed reactions dependent largely on material interests. The wealthy merchants, hopeful of trade concessions, and the clergy, hopeful of patronage and toleration, if not support, favored cooperation with the new government. The middling and lower classes, however, resisted the takeover and felt themselves betrayed by that Dutch elite. Balmer convincingly argues that Dutch cultural and religious identity combined with economic dissatisfaction to structure and reinforce a symbiotic ethnic and class consciousness. For Balmer, Leisler's Rebellion reflected this early combination of economic and ethnic interests exploding into political revolution.

The next fifty years saw these two forces further divide the community. An English policy openly supporting Dutch Reformed clergy further coopted clerics and alienated lay persons from those pastors. Moreover, the newly formed Anglican Society for the Propagation of the Gospel filled in great gaps in service left by an inadequate supply of Dutch ministers, gathering many Dutch congregants into the Anglican fold. In the face of these defections, the Dutch clergy began to resist the English, but their numbers were too few, their efforts too late.

Meanwhile, an expanding frontier culture encouraged pietistic evangelicalism in the disaffected classes. Although significant pietist efforts and schisms afflicted the church before 1720, the charismatic Theodore Jacob Frelinghuysen became the critical revivalist in the middle colony Dutch community. Here Balmer colorfully portrays a church torn between Old World institutional loyalties and New World charismatic authority. Although he seriously understates the passionate rhetoric and style, some would say wild antics, of Frelinghuysen, Balmer does convince his readers of the fierce loyalty of the frontier Dutch to their new leaders.

Balmer concludes with the ultimate victory of English culture. In outlying areas, popular disaffection with the established clergy and the full discontent of the lower classes found expression in the Great Awakening. While the orthodox worked in conjunction with Anglicans, for example, placing a Dutch Reformed theologian on the faculty of King's College, the pietists moved into English evangelical culture. By the time of the revolution, the Dutch language was disappearing, and Dutch religionists of various persuasions were hiring English-speaking ministers. The completion of Anglicization was evident in revolutionary politics, where the orthodox and the Anglicans favored the

loyalists, and the evangelicals attended the patriots' cause.

Balmer's discussion of the cooptation of the Dutch elite and the decline of their ethnic distinctiveness is a fascinating, convincing piece of cultural analysis. Unhappily, I found his discussion of pietistic religion less successful. Why would a society, which Balmer characterizes as lawless and anti-authoritarian, encourage a religiosity emphasizing personal piety and "godly living" (p. 68)? Even more puzzling is how Dutch pietists could establish such strong ties with English evangelicals when, fifty years before, they had defined, at great cost, a separate cultural heritage. That process of change is delineated in stark detail, but the overriding influences at work are only vaguely identified and connected. Does this represent the ultimate import of class versus ethnic identification? Or does the majority always win, but through a variety of processes? Of course, no one really expects answers to such questions; historians will debate cultural assimilation and class antagonism for decades to come. But it is good to see such questions asked.

In the end, Balmer's narrative of the little-known Dutch experience in the Middle Colonies renders this book a useful addition to the literature. More important, his work on Anglicization, the capitulation of the Dutch elite for gain, and the obstinacy of the middling and lower classes in refusing to give up their cultural integrity raises questions and suggests patterns that specialists in the colonial period cannot afford to ignore.

MARILYN J. WESTERKAMP
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DANIEL B. THORP. *The Moravian Community in Colonial North Carolina: Pluralism on the Southern Frontier*. Knoxville: University of Tennessee Press. 1989. Pp. xi, 263. \$29.95.

The Old Salem restoration in Winston-Salem, North Carolina, attracted the attention of both scholars and casual visitors to the German-speaking Pietists of the *Unitas Fratrum* or Moravian church who settled in 1753 on the one-hundred-thousand-acre Wachovia Tract and established Salem as their commercial center a few years later. These Pietists came for privacy and profit, and both required good relations with their neighbors.

Daniel B. Thorp's superb study of the first nineteen years of the Moravian settlement is social history at its best. Thorp has definitively put to rest the notions that Wachovia was established from missionary motives and that relations with outsiders were conflictive, impressions that were given by Adelaide Fries's publication of "a tiny portion" (p. 208, n. 8) of the sources in the eleven volumes of *Records of the Moravians in North Carolina* (1927-69). The vast quantity of material in Moravian archives at Salem and the Bethlehem, Pennsylvania, mother colony enabled him to know every

detail about the fewer than four hundred people who formed this community and their dealings with their neighbors, although attitudes of these non-Moravian neighbors must be gleaned from odd scraps of evidence. Thorp analyzes in a graceful and readable narrative style the structures that held the community together and their commercial, political, and religious contacts with other North Carolinians. He asks important questions and locates his subject in the wider European and American context.

Thorp frames his study within the theory of Norwegian anthropologist Fredrik Barth that groups survive in multiethnic societies not by isolation or confrontation but by contact across cultural boundaries. But do Barth's categories really apply to Wachovia? Is not Moravian pluralism as *sui generis* as Moravian pacifism? Thorp describes a group isolated from its neighbors and contemptuous of them. All decisions were made by seven ministers and ministers' wives, subordinated after 1763 to a single administrator and always to authorities in Bethlehem and Europe. Concerned in everything with outward appearances, these leaders stage-managed business relationships and politics, negotiating by preference at the highest levels. They determined the patterns of trade, secured an "Anglican" parish under Moravian control, and succeeded with other exemptions. They defined the cultural boundaries and selected the few individuals authorized to cross them.

Thorp dismisses the contradiction inherent in a church forbidding religious contacts with outsiders and the reversal of this decision in 1759. The missionary efforts of John Utley and others (whose reports were published by Fries) were surely more than window dressing; at the least they resulted in Moravian congregations spread over a sixty-mile circuit around Wachovia. Did not proselytizing add a new dimension to pluralism? An otherwise complete bibliography contains no reference to the parallel material on Pennsylvania Moravians in Sally Schwartz's *A Mixed Multitude* (1988), which is illuminating on this point.

The literature on the Wachovia Moravians is substantial, but this book is the best on the subject and an important contribution to our understanding of the different ways in which ethnic and religious groups learned to live together in the American colonies.

RICHARD K. MACMASTER
Bluffton College

MARVIN S. HILL. *Quest for Refuge: The Mormon Flight from American Pluralism*. Salt Lake City: Signature. 1989. Pp. xxii, 288. \$19.95.

Not every group in American society greeted with enthusiasm the cultural diversity created by constitutional provisions for freedom of religious belief. Ironically, even as guarantees excluding unilateral state-sponsored religious beliefs allowed the establishment of so unorthodox a religion as Mormonism, the Latter-

day Saints recoiled from the religious pluralism that gave them life. A society in the sorts of upheaval typical of Jacksonian America did not fit the Mormons' view of God's orderly kingdom, and they found no place for themselves to live comfortably within its confines. Marvin S. Hill argues that the impelling force behind the Mormon exodus from civilized America to the granite-rimmed valley of the Great Salt Lake in the sparsely inhabited American West was as much a push emanating from a genuine fear of pluralism as it was a pull toward creating the "city on a hill" that could stand as an example of God's intended way. Unable to deal effectively with the chaotic administrative situation inevitably accompanying new movements of this sort, the Mormons, Hill argues, fled west, convinced that Gentile wickedness drove them out. Visionary myopia obscured the Mormons' view sufficiently so that they were unable to see that they had indeed exceeded the tolerances of religious pluralism by advocating bloc-voting, controlling the press, maintaining a private standing army, requiring allegiance to a theocratic political kingdom, and, most offensive of all, practicing plural marriage. In a most unusual paradox, the Latter-day Saints were welcomed back to the pluralistic fold in the American religious mosaic only when they eliminated their unique religious stance and became one denomination among many. And they could make their distinctive contribution to American religious freedom only when this happened.

Hill's book tells the Mormon story from a new perspective, and this is its strongest feature. The extensive notes are a model of precision and buttress his work with as much detail as if they were preparation for arguments before a tribunal, perhaps a necessary feature of Mormon historiography at the end of the 1980s. The book is smoothly and precisely crafted with theology, history, and philosophy interwoven. For those already familiar with the basic account of the Mormon work, the introduction and first chapter effectively set forth the book's thesis, drawing the Mormon experience into the larger picture of the religious experience in America rather than writing it as exclusionary to the broader themes as Mormon history is so often presented. Twenty-five years in the making, this book adds yet another facet to understanding the American religious experience.

VALEEN TIPPETTS AVERY
Northern Arizona University

CURTIS D. JOHNSON. *Islands of Holiness: Rural Religion in Upstate New York, 1790-1860*. Ithaca: Cornell University Press. 1989. Pp. xiv, 214. \$27.95.

It has been forty years since Whitney Cross's *Burned-Over District* called attention to religious revivalism in early nineteenth-century New York State. More recently, case studies by Paul Johnson on Rochester and Mary Ryan on Oneida County linked this phenomenon to an emerging middle class and to gender.

Now Curtis D. Johnson adds this solid case study on Cortland County. The book shares much with its predecessors, but it also stands apart. Cortland County was rural, not urban, when the revival came, and Johnson finds the class-oriented explanations of Paul Johnson and Ryan unsuitable here. Indeed, Curtis Johnson argues that the revival is best explained not by socioeconomic factors at all but by ideology.

Johnson's thesis is that revivalism was the religious manifestation of a general, postrevolutionary trend toward "republican individualism." Revivals occurred as people came to believe that they actually had free choice and control of their own spiritual destiny, and Charles Finney's Arminianization of Calvinism must be understood in this light. The revivals transformed Cortland County's congregations from "islands of holiness" (corporate churches held together by a communitarian ethos and by disciplinary practices that reinforced purity, group identity, and separation from the world) to "modern" churches characterized by an individualism that both undermined and rendered archaic earlier practices of church discipline. Ironically, the revivals left less culturally significant and less spiritual churches.

Although Johnson tries hard (probably too hard) to separate himself from historians who emphasize socioeconomic rather than intellectual factors, readers will find a great deal of sophisticated social history in the book. As he explains, Arminianization may have inspired the revival, but gender, denomination, and institutional circumstances gave it shape. He includes, for instance, a good chapter showing how institutional factors, such as competition for members, affected the revivals.

Women became important leaders in the revival, partly because society restricted how they could express republican individualism. Male autonomy could be directed into business and politics, but female autonomy found its outlet in home and church. In fact, the community's leading men did more than merely ignore revivalism; they resisted it. Elsewhere, middle-class businessmen may have seen revivalism as a way to impose social control on workers, but in Cortland County socially prominent males feared that revivalism's behavioral restrictions would compromise their own autonomy. It was only after churches were weakened beyond any ability at all to impose social control that these religious "minimalists" considered them safe to join.

Johnson does have a tendency to present other historians' views (especially in his chapter "The Failure of Economic Explanations of Rural Revivalism") in such extreme forms that one wonders whether their proponents would recognize them. One could say the same about his portrait of early nineteenth-century Calvinism, which was more diverse and often more moderate than he allows.

But this is a good book, rich with many more insights than can be included here. Those who read the pre-

ceding studies on the Burned-Over District will want to read this one, too.

CHARLES D. CASHDOLLAR
Indiana University of Pennsylvania

ALBERT FURTWANGLER. *American Silhouettes: Rhetorical Identities of the Founders*. New Haven: Yale University Press. 1987. Pp. viii, 168. \$17.50.

Albert Furtwangler has written an intriguing study of the rhetorical identities and personal political styles of the most eminent leaders of the American revolution. He takes his title from the eighteenth century's fascination with the craft of silhouette cutting, fragile paper constructions presenting an individual's profile and offering suggestive clues to his or her essential character. Rather than paper, Furtwangler works with selected written texts of five of the nation's founders, together with the dramatic historical moments that provided their context, to describe the founders' different rhetorical stances and tease out the personal political style that each created in the midst of revolutionary change.

The book's chapters offer a series of compelling vignettes, cultural snapshots of the young Franklin turning the sharp style of John Addison and Richard Steele to his own purposes in his "Silence Dogood" essays; John Adams contesting Daniel Leonard's "Massachusettsensis" in his lawyerly, tightly argued "Novanglus" pieces; Washington finding in a performance of Addison's Cato a model for his own heroic sacrifices and offering additional self-definition, with Madison and Hamilton's conflicting help, in his Farewell Address; Jefferson in his famed correspondence with Adams reflecting on what their generation had wrought and how he and the New Englander had differed; and John Marshall's towering, anti-Jeffersonian decision in *Marbury v. Madison*.

Deft, insightful studies of character and context these chapters certainly are. In their reading, even the experienced historian finds fresh insights into these overly familiar characters. In the end, however, the book leaves one asking whether more about the revolution's overall meaning may not be drawn from these lives and concluding that the book's parts offer more than the whole.

The reader does carry away a clear sense of the variety of political personalities among the revolution's inner circle, the almost total absorption of private identity in the public drama of revolutionary events, and the extent to which individual identities were formed in the crucible of personal conflict: Franklin against Cotton Mather, Adams against Leonard, Jefferson against both Adams and Hamilton, and Marshall against Jefferson. In the end, that may be enough of general consequence for a slender volume to convey.

JOHN HOWE
University of Minnesota,
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CHARLES A. MILLER. *Jefferson and Nature: An Interpretation*. Baltimore: Johns Hopkins University Press. 1988. Pp. xii, 300. \$29.50.

This new book will inevitably be compared with Daniel Boorstin's classic study *The Lost World of Thomas Jefferson* (1948), published more than forty years ago. Like Boorstin, Charles A. Miller believes that assumptions about nature and the material world shaped Jefferson's way of thinking about everything, including politics, and thus gave whatever structure and coherence there was to his thought. Both authors suggest—Miller more explicitly and persuasively—that Jefferson's reliance on nature to define meaning and purpose in human affairs distinguished him from many of his contemporaries, including James Madison, his closest partner in politics. And both scholars are generally critical of Jefferson's way of thinking. Boorstin presented a formal critique of Jeffersonianism as philosophically superficial and inadequate. Miller is far less direct and systematic in articulating his reservations (which seem quite similar to Boorstin's), although occasionally, as in his discussion of Jefferson's approach to international relations, he can appear to be mocking his subject. Comparing Jefferson to Madison and John Adams, Miller ultimately finds the great Virginian, for all of his intellectual brilliance and versatility, "the least of the three as a philosopher" precisely because he failed to recognize "that nature was uselessly ambiguous, or too simple and too simplifying an idea on which to base arguments about society" (p. 254).

Part of the novelty of Boorstin's book was his unorthodox approach to intellectual history. Seeking to reconstruct a lost and therefore exotic world of experience, he concentrated on presenting and bringing to life for modern readers the ideas themselves, caring not a whit about traditional scholarly concerns, such as from whom Jefferson (as well as other members of his circle of thinkers) drew those ideas. Miller goes beyond merely re-creating an intellectual world, and his work is a more conventional intellectual history. In addition to describing and analyzing Jefferson's ideas, he explores in some depth his subject's reliance on (and revision of) other writers and thinkers, for example, the Roman philosopher-poet Lucretius, whom Miller presents as a key of sorts to understanding Jefferson's view of nature and perforce the world.

Divided into three parts labeled "Being," "Value," and "Action," Miller's book, described simply, offers a synthetic catalog of the manifold ways in which Jefferson understood, used, and cleverly manipulated the terms "nature" and "natural." Indeed, Miller's account of the background, context, and application of Jefferson's understanding of nature is now the most thorough and reliable treatment of the subject we have. And, although much of his discussion covers relatively familiar ground (given the extraordinary volume of the literature on Jefferson, this seems unavoidable) and is also occasionally weak on relevant contexts other than the intellectual, in toto this study offers a persuasive

defense of the argument that, as Miller puts it at the beginning of his concluding chapter, "nature was Jefferson's myth for all purposes, a flexible idea that gathered together his deepest beliefs" (p. 251). Exceptionally clear, thoughtful, and accessible to specialists and nonspecialists alike, this book is a valuable addition to studies of Jefferson.

DREW R. MCCOY
Clark University

SARAH BURNS. *Pastoral Inventions: Rural Life in Nineteenth-Century American Art and Culture*. (American Civilization.) Philadelphia: Temple University Press. 1989. Pp. xi, 377. \$39.95.

This handsome book takes an important step toward the goal, shared by many cultural historians, of interrogating visual evidence with the same thoroughness and subtlety that we have learned to apply to the written word. Sarah Burns has compiled a diverse and wide-ranging collection of visual sources depicting American rural life and examines them in the light of current scholarship in social, economic, political, literary, and intellectual history. She explicates familiar popular images—the yeoman farmer, the Yankee, the barefoot boy—as well as more novel subjects such as landscape architecture, the mechanization of farming, and the contrast between rural and urban life. In the process, she offers a convincing argument that idealized and nostalgic representations of farm life in nineteenth-century America served as an important focus for anxieties arising from rapid social, economic, and political change, especially in the years immediately following the Civil War.

One of the book's greatest strengths is the way in which its visual sources cross the lines that so often have separated art history from the study of popular culture. Easel paintings by well-known figures such as William Sidney Mount, Eastman Johnson, and Winslow Homer are considered beside Currier and Ives prints, *Harper's Weekly* illustrations, political cartoons, and photographs from popular books. Despite these accomplishments, however, the book has some serious weaknesses. Burns's analysis of individual examples lacks depth and detail, as if she is afraid to slow the book's pace for the sake of greater complexity. Some of the objects considered, such as Mount's *The Painter's Triumph* (1838), George Inness's *Peace and Plenty* (1865), or Homer's *Snap the Whip* (1872), could sustain much closer scrutiny, which would advance her argument in greater depth.

The book is marred by other omissions. In this examination of the significance of rural life in the popular imagination, we find almost nothing about women. The author presents "hicks," "barefoot boys," "yeomen," and "grandfathers" but offers no recognition that these terms are themselves highly revealing. Similarly, she says nothing at all about race, even in the sections that deal with the anxieties of the post-Civil

War years. More generally, the book does not go far enough in considering the social dimensions of the production and consumption of the visual images it considers. What was the social and intellectual standing of the artists who made these images? Who bought them? Who viewed them and under what circumstances? Such questions would help us better assess their cultural significance.

Finally, the book is disappointing to the cultural historian who seeks insight into the complex interaction of economic and social conditions, popular culture, and artistic expression. Although the author brings these elements into juxtaposition, she gropes in vain for a consistent model to explain their interrelationship. Sometimes she explicates an artistic image and looks for echoing examples in literature and political rhetoric; at other times she seems surprised that the fictional "idyll" does not correspond to "true historical circumstances" (p. 50); and occasionally she suggests that "crass economic motives" influence artists in some unspecified kind of "subliminal" fashion (p. 223). Furthermore, the book's thematic structure fosters repetition and makes it hard to trace changes in rural iconography over time. This book has staked out a rich field and opened up new vistas, but it leaves to others the task of thorough cultivation.

JOY S. KASSON
University of North Carolina,
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WILLIAM J. GILMORE. *Reading Becomes a Necessity of Life: Material and Cultural Life in Rural New England, 1780-1835*. Knoxville: University of Tennessee Press. 1989. Pp. xxvii, 538. \$49.95.

This study focuses on the reading habits of people living in the southern half of a Vermont county bordering the Connecticut River. William J. Gilmore mines a set of documents consisting of 386 estate inventories probated between 1780 and 1835 that list books by author or title. These inventories represent roughly two-thirds of all inventories of the period originating in the district and probably offer a fair portrait of the propertied part of the population. The books and their editions have been exhaustively identified by Gilmore, enabling him to classify them by topic and "persuasion" with respect to world views.

Gilmore provides a richly complex geographic and economic context for these readers and their books: mapping roads and settlements, locating primary and secondary sites of commercial activity, and identifying social and physical "habitats" and their networks of communications and exchange. He tracks the economic development of the area from settlement, 1760-95, through the initial stages of commercialization, 1795-1815, and then to a leveling-off that signaled the onset of economic stagnation. Gilmore similarly portrays the progress of print culture in the "Upper Valley" by tracing the growth of signatures versus

marks, the spread of schooling, and expanding "zones of access" to printed materials.

By investigating the socioeconomic characteristics of book owners, the author hoped to link these to the owners' reading choices and, thus, to the *mentalité* in an increasingly commercial world. Of course, inventories of books acquired over shorter or longer lifetimes cannot describe what was being read at any one time, and that is why titles carried by storekeepers and distributors are important to his story. Yet, in carrying out his analysis of the probate lists, Gilmore inexplicably treats the approximately fifty-five hundred volumes as a single set with no time dimension at all.

This is an immensely ambitious book that is not wholly successful but is nevertheless important and timely. Space does not permit the sustained analysis the author so richly deserves, and what follows may exaggerate the book's drawbacks. First, and most critical, is its awkward organization. Despite an elaborate apparatus of parts, titles, and subtitles, the chapters simply do not march along in any logical order with the consequence that topics appear and reappear quite unpredictably. Discussion of the spatial distribution of the population, sites of commerce, and communications networks should have been directly and systematically related to habitat and the distribution of books and libraries. Similarly, the subject matter of the inventoried books and their distribution among specific types of families deserved a single, integrated essay instead of being scattered over three hundred pages. The presentation of numerical evidence is often chaotic and consistently incomplete; the frustration caused by inadequate presentation of evidence is exacerbated by the placement of the notes at the end; finally, all too many of these notes turn out to be parenthetical rather than supportive or illustrative of important points in the text. The author and the publisher would have benefited by consulting an expert in quantitative analysis, such as Lee Soltow or Maris Vinovskis, who is also familiar with the kinds of materials used in this book.

These shortcomings seriously hamper one's understanding and appreciation of the book's very real strengths. The author's goals are truly noble, the breadth of the research is impressive, and many of the ideas are fresh and stimulating.

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GEORGE B. KIRSCH. *The Creation of American Team Sports: Baseball and Cricket, 1838-72*. (Sport and Society.) Urbana: University of Illinois Press. 1989. Pp. xiv, 277. \$27.50.

In this study, George B. Kirsch offers a comprehensive, comparative analysis of the early years of baseball and cricket. Kirsch examines why baseball became the national pastime and cricket faded into obscurity. In 1850 both games experienced a burst of popularity, and it

was unclear which one American players would favor. Kirsch discusses five characteristics that determined the fate of the two games.

First, Kirsch challenges the view of most sports historians that ball playing was not a very significant feature of early nineteenth-century American athletics. He presents evidence that a great number of adult men as well as children participated in sports despite the rural nature of society, the tradition of individualism, and religious objections to such amusements. That more young children played "old cat" or "base" gave baseball an edge over cricket in becoming the national pastime. A second factor was the availability of playing fields and equipment. As cities grew, vacant lots for any ball game became harder to find. But cricket fields were even scarcer, as they required a manicured and smooth surface.

Although cricket was not rejected simply because it was British, the fact that the British controlled the game in an effort to preserve their own cultural identity made it less accessible to the American masses. British players tended to monopolize the sport, keeping American players from developing a talent for and interest in cricket.

Fourth, Kirsch argues that the structural characteristics of the two games were important determinants. Baseball rules were simpler and easier to learn, especially since many young men had played variations of the game in their youth. Baseball had a faster pace, allowing more players to join in the action. Perhaps most important, a baseball match could be completed in two to three hours, as opposed to one to two days for a full game of cricket. Time-conscious players and spectators were unwilling to allot so much time to recreation.

Finally, by the mid-nineteenth century, cricket was already well established with formal rules and a governing body in England. Baseball was still in its early stages of transition from a folk to a modern game. The British were unwilling to alter the rules of cricket to fit American circumstances, but baseball was adapted to fit the needs of the industrializing country. Baseball's capacity to excite players and spectators was not matched by cricket.

Kirsch also examines how these team sports fit into the cultural, social, and political life of mid-nineteenth-century America. The intense partisanship that frequently marked baseball matches reflected the contentiousness that plagued most American cities. Urban rivalries for railroads, industry, and commerce were also played out on baseball diamonds. Public officials used baseball clubs to boost their own standing within the community. Perhaps the most dramatic example of a baseball team's political influence occurred in Boston. In 1869 the Lowells and their friends backed candidates who pledged support for athletics generally and new baseball grounds in particular. The baseball party ran tickets marked with large red balls and swept their candidates into office.

Drawing on contemporary newspapers, scorebooks,

and sports journals, Kirsch offers a detailed look at the men and women who played and watched cricket and baseball.

JANET BRUCE CAMPBELL
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Shawnee Mission, Kansas

JAMES A. HIJYA. *J. W. De Forest and the Rise of American Gentility*. Hanover, N.H.: University Press of New England, for Brown University Press, Providence, R.I. 1988. Pp. xi, 177. \$20.00.

In recent years American cultural historians have devoted a good deal of attention to the question of how American writers and artists have viewed the masses, who have not always valued their work. James A. Hijiya's brief intellectual biography of John William De Forest makes an important contribution to that subject. Drawing on De Forest's unpublished correspondence as well as his numerous published works, Hijiya shows that from an early age De Forest was taught to achieve self-esteem by cultivating "gentility," an ideal that involved an amalgam of taste, intellect, honor, courage, and physical vigor. De Forest's entire literary career can be seen as a continuous effort to preserve his self-respect in the face of doubts about the legitimacy of writing as a profession and repeated failures to achieve commercial success with his work.

If, however, De Forest remained committed to gentility, the nature of that commitment and his view of the common people evolved in accordance with changes in his own self-image. According to Hijiya, De Forest's life and thought should be divided into three phases. In his "Whig" phase, encompassing the period from his birth in 1826 to 1860, De Forest embraced the notion that Protestant, Anglo-Saxon culture was superior to its rivals. Although he expressed concern that modern civilization bred people without great capacity for heroism, his opinion of the common people was that they were capable of acting virtuously as well as vulgarly. The work he published in this period sold so poorly that he contemplated abandoning fiction. His service as an infantry officer in the Civil War and as an official in the Freedmen's Bureau, however, enabled him to practice the martial virtues that he admired, and this gave him the confidence to resume his career as a novelist. It was in this short, "Republican" phase of his life, which spanned the years from 1861 to 1868, that De Forest published *Miss Ravenel's Conversion* and most of his other best literary efforts. Hijiya convincingly argues that De Forest's interpretation of the war as the heroic triumph of democratic individualism over a more primitive, undemocratic way of life made this period the most egalitarian phase of his career. In turn, De Forest's optimistic assessment of the common people gave him the equanimity necessary to depict characters and events in a realistic manner. Neither his egalitarianism nor his literary realism, however, persisted. During the "Mug-

wump" stage of his career, which lasted from 1869 until his death in 1906, De Forest's failure to achieve great literary success drove him to conclude that his society appreciated neither gentility nor the literary productions of its representatives. At the same time, his novels increasingly moved away from realism toward the use of cardboard characters expressing his elitist views of human nature.

The title of this book is somewhat misleading. Actually, Hijiya is concerned with the role of gentility in De Forest's thought rather than with the rise of that ideal. One can also question the appropriateness of using political party designations to describe the phases of a career of someone who was not terribly concerned with electoral politics. Still, Hijiya's interpretation of De Forest's thought is illuminating, and his claim that De Forest succeeded in practicing literary realism only during the brief, "democratic" phase of his career is both suggestive and convincing.

JON H. ROBERTS
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NORMA LOIS PETERSON. *The Presidencies of William Henry Harrison and John Tyler*. (American Presidency Series.) Lawrence: University Press of Kansas. 1989. Pp. xiv, 329. \$29.95.

This volume in the American Presidency Series is a welcome study of the administrations of William Henry Harrison and John Tyler. Norma Lois Peterson views these years primarily as a struggle for supremacy between the executive and legislative branches of government, and her sympathies plainly rest with the men in the White House—Henry Clay emerges as the chief villain. Harrison was an ideal candidate for the Whigs in 1840; a military hero, he was pledged to accept the will of Congress even though personally opposed to a national bank. Tyler, who broke with the Democratic party over Andrew Jackson's "executive usurpation" of power, was selected as his running mate to balance the ticket geographically. No thought was given to the succession; after all, every president had completed his term. But Harrison, of course, died a month following his inauguration, and Tyler became the first "accidental president."

Peterson needs only a dozen pages to examine the presidency of Harrison and understandably focuses on his cabinet appointments. His brief administration naturally remains one of unfulfilled promise; nonetheless, "indications are that he would have grown more unhappy and refractory as the cabinet and Congress attempted to limit his executive power" (p. 41). It remained for Tyler to enter into open political combat with Congress.

Tyler believed that the office of the president devolved on him with Harrison's death, and after the cabinet concurred he took the presidential oath. The precedent thus was established for an orderly suc-

cession in the executive branch. Yet Tyler's relations with Senator Clay and Congress were anything but orderly. Tyler is portrayed as a besieged politician (ultimately abandoned even by the states' rights Virginians) who valiantly defended the office of the presidency from the assaults of his fellow Whigs. Clay, in contrast, is a bitter man; resentful at being passed over for the nomination, he "was determined to rule without benefit of election" (p. 31). Indeed, Peterson characterizes Clay as a petulant obstructionist who strove to tear down the presidency in order to further his own ambitions and frustrate the plans of Tyler. The first point of contention was the bank issue. Despite the pleas of his cabinet, Tyler vetoed the various bank bills passed by Congress. More than ideological differences were at stake. Clay "wanted to bring down Tyler" (p. 80) more than he wanted a bank, and "what really angered congressional Whigs was Tyler's thwarting of their determination to control the chief executive and to destroy, for all time, presidential usurpation" (p. 89). Regarding executive responsibility, Tyler simply "did not handle the bank issue with aplomb" (p. 92).

While domestic legislation was bogged down in the struggle between Tyler and Congress, the most significant achievements of this administration came in the field of foreign affairs. Secretary of State Daniel Webster is given due credit for improving relations with Great Britain, while Tyler remains curiously at the periphery of American diplomacy. Peterson discusses the negotiations leading to the Treaty of Washington and the Oregon boundary dispute in some detail, but the president steps to center stage solely to announce the "Tyler Doctrine" (the United States will brook no foreign interference with the Hawaiian kingdom). Tyler exerted still less influence over international affairs after Webster's resignation. A succession of secretaries of state followed their personal agendas—especially regarding Texas annexation—and "they played 'fast and loose' with Tyler, who naively was unaware of what was happening" (p. 187). The president, in fact, is lost from the narrative for pages at a time, although he temporarily took charge following the Senate rejection of the annexation treaty. Three days before he left office, Tyler had to be persuaded by his cabinet to sign the joint resolution that brought Texas into the Union. This is scarcely the portrait of a strong executive in command of his administration.

Peterson diligently perused the important secondary sources pertaining to this period and offers historiographical analyses of the major issues and events; nevertheless, this work would have been strengthened by additional research in primary sources. Few references are made to the papers of Harrison or Tyler (mention is made that some of the correspondence has been destroyed), and the *National Intelligencer* is the only newspaper frequently cited. Peterson also tends to present facile characterizations. "Locofocoism" is merely "the radical movement in the Democratic party" (p. 58). More frustrating to the reader, both Tyler and Roger Brooke Taney "were devoted to the South

... while neither was zealous in defense of slavery" (p. 269).

Despite this quibbling, Peterson's well-written study is a worthy contribution to our understanding of the administrations of Harrison and Tyler. The author presents a clear thesis: "Tyler's presidency was flawed by the imperiousness of Clay" (p. 262). And yet twenty-two men variously served in the six cabinet positions under Tyler; the six nominations Tyler made for the Supreme Court resulted in a solitary confirmation; and, for the first time, Congress overrode a presidential veto during this administration. At times, Peterson concedes, Tyler "seemed to lose control of his government" (p. 266). Clearly, the image of John Tyler as a vacillating and ineffective president will not be dispelled by this book.

WILLARD CARL KLUNDER
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JOHN NIVEN. *John C. Calhoun and the Price of Union: A Biography*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1988. Pp. xvi, 367. \$24.95.

John C. Calhoun has not lacked for scholarly attention, but, because the most recent biography of Calhoun is now almost thirty years old, a fresh look is overdue. That is especially true given the explosion of compelling recent scholarship about the slave system, the social and intellectual scene in South Carolina, the crosscurrents between the nation's powerful partisan political structure and its intense sectional tensions, and the deeply ingrained republican ideological tradition, all buttressed by the continuing letterpress publication of Calhoun's voluminous papers. John Niven brings to his study of Calhoun long-standing credentials as a biographer of mid-nineteenth-century political figures. His particular strengths are his handling and coherent organizing of a mass of detail and his clear presentation of Calhoun's private and public lives. Niven recounts much that is familiar, from Calhoun's war hawk days to his role in the crisis of 1850 to the vagaries of his family and social situation. Niven works hard to uncover Calhoun's social and political worlds and to explore all that went on, largely from the South Carolinian's perspective. He focuses particularly on the activities of the political elites in Washington and Charleston. He is much less interested in the broad socioeconomic movements and shifting political structures that underlay and interacted with the leadership's dealings. In covering Calhoun's long career, Niven rarely leaves his subject's side for an extended consideration of larger developments.

Niven reinforces our view of Calhoun as the strongly committed, crusading, single-cause independent who was isolated from many of the political currents that were transforming America, skeptical of popular power, deeply suspicious of the emerging partisan political style, and hypersensitive to sectional dangers.

Niven suggests that the primary wellspring of Calhoun's behavior was his "latent insecurity" (p. 16), which reinforced and expanded his excessive fears for the well-being of his class, section, and nation and led him into a consistently defensive political posture. Calhoun's writings in the late 1820s expressed a constant tone. In them, he "set forth the grievance of the planter class in his state. He also gave vent to his own frustrations—his deep feelings of insecurity for his own future and the future of a social order and a nation that he felt were in deadly peril" (p. 137).

Calhoun was an exaggerator and destabilizer in debate; he was a political polarizer, never a consensus builder. Loyal to a particular image of the Union, he too often, in Niven's view, led matters into explosive situations. "Such a suspicious, insecure person, gifted as he was, with an unusually active mind, a fine sense of logic, a debater's skill of a high order, and a passion for self-justification, could and usually did find a conspiracy in the actions of others that would alter the social and political norms he valued" (p. 202). He had little use for or understanding of the new mediating political institutions that were developing around his adversary, Martin Van Buren. Committed to building consensus and coalition, those institutions were designed to bring under control the excesses of American pluralism and to prevent the paranoia of any group and the rhetorical exaggeration of its leaders from posing threats to the nation.

Unlike Calhoun, Niven rides few hobby horses. His treatment of Calhoun is deliberate and straightforward, and he does not press his interpretations too hard or push any quarrels he has with other scholars to a confrontational level. His approach to controversial matters is brief, prudent, and indirect. He allows the detail he has amassed to tell the story as he sees it. The book, therefore, fills a particular niche: it is not as detailed or as broad in scope as Charles Wiltse's still quite valuable three-volume life of Calhoun, yet it is more thorough, if less pointed, than the single-volume interpretations of the South Carolinian by Richard Current and Gerald Capers.

JOEL H. SILBEY
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ROBERT W. JOHANNSEN. *The Frontier, the Union, and Stephen A. Douglas*. Urbana: University of Illinois Press. 1989. Pp. xiii, 311. \$34.95.

This book draws together fifteen lectures and articles written by Robert W. Johannsen. Scholars of the period will find nothing new in this volume, because all but two of these pieces have been published before in various journals and collections. But the idea of the book, no doubt, was simply to draw together the scattered works of a long and productive career under the main themes that have guided that career. Johannsen himself claims little for these essays. Although he acknowledges that they do not settle any of the great

problems of mid-nineteenth-century historiography, he does express his hope that "they will make the pursuit of those vibrant and troubled years a little more interesting to the pursuer" (p. xii).

The essays are grouped under three main headings. The first group treats the sectional crisis from the perspective of the Pacific Northwest. The second group analyzes various aspects of Stephen Douglas's role in American politics, and the third gives some attention to Douglas's historical and historiographical rival, Abraham Lincoln.

Johannsen was born and educated in the Pacific Northwest, and in many ways the insights and the flaws in his vision of American history may derive from that fact. In all of his essays one has the sense of viewing the coming of the Civil War from a kind of moral distance, as it was viewed, according to Johannsen, by the people of the Pacific Northwest. Primarily interested in achieving greater control over their own territorial governments, these distant Americans never responded with enthusiasm to northern radicals who attacked slavery or to southern radicals who were willing to break up the Union to protect it. Douglas, who promoted the idea of local self-government in the territories and who was similarly tone-deaf to the moral importance of the slavery issue, seemed their ideal spokesman.

Johannsen attempts to defend Douglas from those who would see him as a defender of slavery and the South. Rather, as Johannsen would have it, Douglas was one of the few men of his day who was "thoroughly national" (p. 206). If Douglas had a flaw, it was merely his determination to remain a romantic nationalist while all others were adopting sectional positions on the slavery issue. And Douglas was not an unprincipled politician whose craving for office guided his actions in the 1850s. According to Johannsen, he was deeply and sincerely committed to America's mission, which, for him, was bound up with the westward march of American institutions and American progress. Slavery was a distinctly secondary issue, a "distraction" to be "adjusted" so that "the nation could get on with its more important business" (p. 98).

The essays on Lincoln are, not surprisingly, the weakest and least satisfactory in the collection. Although Johannsen seeks to come to terms with Douglas's great adversary, his appreciation of Lincoln seems perfunctory. It is the kind of appreciation that Douglas himself might have had: respectful of Lincoln's achievements but never quite persuaded that his course was the proper one for America.

For Johannsen, it was Douglas, not Lincoln, who captured the spirit of the mid-nineteenth century (p. xii). Perhaps this assertion reveals the key to Johannsen's thought. He, like his subject Douglas, seems caught in the era of "Young America." He is most insightful and eloquent when discussing the late 1840s and early 1850s. And he, again like Douglas, seems less comfortable with the moral fervor of the years that followed. Douglas may indeed have captured more of the spirit of 1848 than Lincoln, but he also failed to

grasp something very important in the American spirit, something that would propel his adversary to the White House and to greatness.

These graceful essays may not add profound insights to the historiography of our nation's greatest crisis, but all who read them cannot fail to be educated by them. Douglas's view of America's mission may have been cast aside in 1861, but no historian has ever made it more understandable or more appealing. Those who pursue the history of Douglas's troubled era will indeed find it "a little more interesting" after reading this book.

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WILLIAM M. MATHEW. *Edmund Ruffin and the Crisis of Slavery in the Old South: The Failure of Agricultural Reform*. Athens: University of Georgia Press. 1988. Pp. xiv, 286. \$35.00.

William M. Mathew has reexamined the role of Edmund Ruffin in economic change in the antebellum South. After detailing Ruffin's life, Mathew spends the majority of the book examining Ruffin's proposals for reforming agricultural practices. Mathew's central thesis is that slavery was not sufficiently adaptable in the older farming regions along the Atlantic coast to allow the reforms that he believes were necessary. Mathew seconds Ruffin's plan to reduce soil acidity through the use of marl in conjunction with organic supplements and a more sophisticated rotation system along the lines of that developed in English agriculture.

Mathew devotes a substantial and surprisingly interesting discussion to the supply of marl in the South, as well as the costs of digging, shipping, and spreading it. He also investigates the abilities and instincts of southern planters to use it. The actual result, however, was that probably no more than 5 percent of southern farmers, mostly in Tidewater Virginia, adopted the recommended practices. The remainder of southern farmers, to Ruffin's and Mathew's displeasure, continued using "land killing" practices that depleted the fertility of the soil, and then they moved on to new acreage.

Mathew goes out of his way to contrast his view of the necessary reform with that of Robert E. Fogel and Stanley L. Engerman, particularly as reported in *Time on the Cross* (1974). He believes that their method adopts a short-term view that slights the necessity of longer-term adaptation. Here Mathew is misled by the common misconception that modern economic theory overemphasizes immediate returns. Rather, it uses a weighted average of the present to distant returns determined by the participants themselves. Southern landowners had good reasons to incorporate future uses of their land in their practices; it directly affected its value. In retrospect their practices may seem short-sighted to historians, but they are not that far from what most generations would choose.

The danger of rejecting Fogel and Engerman's direct approach is illustrated in Mathew's own effort. He constructs an alternative, roundabout framework whose increased linkages raise the opportunities for error. The danger of his exercise is illustrated by a simple observation, namely, that the cost that most southern farmers would have had to spend to add marl and complementary improvements simply did not provide a compensating increase in the value of land. The significance of this measure can be understood with much less sophistication than that employed by Fogel and Engerman; it is basic economics, which has been available for several generations. The resources required to implement Ruffin's suggested system of agricultural reform were too expensive and thus not the best strategy for the South. In his detailed development of agricultural reform, Mathew runs into a cul-de-sac that a simple index would have highlighted. This, in turn, undercuts his view that slavery retarded reform by suggesting that southern farmers were, in fact, making the most of their opportunities.

GERALD GUNDERSON
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RANDOLPH B. CAMPBELL. *An Empire for Slavery: The Peculiar Institution in Texas, 1821-1865*. Baton Rouge: Louisiana State University Press. 1989. Pp. xii, 306. \$35.00.

This book is dedicated to the idea that slavery in Texas was not measurably different from slavery elsewhere in the South and that the institution was important in Texas history. During the troubled colonial period, American settlers, certain that slavery was necessary for rapid expansion, struggled to protect slavery from Mexico's repeated attempts to restrict or abolish it. And, although tension over slavery was not a primary cause of the Texas revolution, guaranteeing slavery was a serious order of business for the new republic.

From the colonial period through the Civil War, Randolph B. Campbell concludes, slavery grew and expanded, earned reasonable profits (largely through cotton), cultivated self-sufficient agriculture, probably retarded commerce and industry, and generally shaped a prosperous society that was governed by a slaveholding elite whose values, anxieties, and views on race and slavery were shared by slaveholders throughout the southern states. Texas slaves lived, labored, and endured a life identical to other slaves in the Old South. They were regulated and protected, exhibited a wide range of behavior, took what privileges they could, and found personal and psychological comfort in family, music, and religion, like most other slaves.

This book is well documented with manuscript collections, newspapers, tax rolls, and census materials. The factual and statistical particulars about Texas slavery and chapters on the colonial period, slavery and the revolution, and slavery's growth and expansion are the freshest parts of the study. The book is informed by

the scholarship of the past fifteen years, but Campbell neither expands nor evaluates it. A study of Texas slavery "is 'new' by definition" (p. 9), proposes Campbell, because his book is the first on the subject.

What is most intriguing about the work is not its major thesis—Texas's unity with the slaveholding South—but its tantalizing pieces of information that suggest the uniqueness of Texas slavery: it was legal to educate and arm slaves; Mexico and Indian Territory provided sanctuary to runaways, who numbered four thousand in Mexico by 1855; Tejanos (Mexicans living in Texas) were thought to encourage runaways and slave "unrest," which brought periodic "vigilantelike reprisals" (p. 218) by whites; 25 percent of Texas slaveholders owned one slave and 50 percent owned four or fewer in the 1850s; most masters lived on their farms and plantations and worked their slaves without overseers; the influx of refugee masters during the Civil War increased the slave population by nearly one-third.

Campbell makes a case for the importance of slavery in shaping Texas's growth and history, and his study of the institution is strongest for the early years and in its statistical information. But he goes astray by limiting his thematic focus to what is familiar about slavery. Texas offers an opportunity to examine how—or whether—distinctive conditions created even subtle differences in the institution and the lives of masters and slaves. That exploration being absent, this book is a well-researched and well-presented traditional state study that supports a substantial body of literature on the subject.

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DAVID E. SWIFT. *Black Prophets of Justice: Activist Clergy before the Civil War*. Baton Rouge: Louisiana State University Press. 1989. Pp. xv, 384. \$35.00.

Traditional consensus on the centrality of church in Afro-American life and history too often overshadows many areas of marginal knowledge about the configuration and dynamics of black churches. Integral questions remain unanswered. Who were, for example, the men that pastored black congregations? Who were the members of the congregations? Why did blacks in some places settle with some congregations as more suitable than others? Why did congregations flourish in some places while failing in others? How did the pastors and congregations affect each other and their communities? Where answers to such questions do appear within an increasingly abundant historiography, connections frequently are missing. The sad result is a black church that can appear as a hollow monument without message or meaning.

David E. Swift searches the message and meaning of black churches by probing the background, commitment, and connections of six ministers whose names should be familiar to students of the antebellum era.

His choices—Amos G. Beman, Samuel Cornish, Henry Highland Garnet, James W. C. Pennington, Charles B. Ray, and Theodore Wright—were men whose talents and toil made them powerful mouthpieces of a gospel of social activism. Each ministered in New York or Connecticut to Presbyterian or Congregational churches, and, as black clergy in predominantly white denominations, each battled the demon of racial segregation within and without his church. These men were eloquent, indefatigable, and visionary on abolition, education, fugitive slaves, and civil and political rights.

Swift's dozen chapters brim with detail milked mainly from abolitionist and church records and blended with secondary accounts. His approach evokes such works as David W. Wills and Richard Newman's *Black Apostles at Home and Abroad: Afro-Americans and the Christian Mission from the Revolution to Reconstruction* (1982) and R. J. M. Blackett's *Beating against the Barriers: Biographical Essays in Nineteenth-Century Afro-American History* (1986). The fact that the cream of Swift's notable contribution lies so heavily in biography reveals the watery base of black church historiography.

Swift's work suffers from perhaps an overly sublime thesis, for he seeks to show basically that, as he puts it, "those having the true spirit of Christianity" can have an impact on their world (p. 356). He poignantly portrays the six ministers' boundless humanity, pervasive persistence, and painful self-sacrifice, but he provides too few demonstrations of significant change that they accomplished. Swift's focus on each of the six contemporaries in separate chapters with an occasional linking chapter also creates a repetition of background that sometimes blocks the narrative flow.

Students of antebellum America or the black church will find, however, more than full measure in Swift's work. It merits a careful reading for the details of denominational and local history alone. His greatest gift is a host of fascinating interconnections that point to an antebellum network of black churches and churchmen that deserves much fuller study.

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Buffalo

DAVID W. BLIGHT. *Frederick Douglass' Civil War: Keeping Faith in Jubilee*. Baton Rouge: Louisiana State University Press. 1989. Pp. xv, 270. \$27.50.

David W. Blight successfully emphasizes the centrality of the Civil War for Frederick Douglass by showing the black reformer's participation in antebellum and war issues and his efforts to preserve the memory of the Civil War during and after Reconstruction. The abolition of slavery must be linked to the abolition of racism, Douglass repeatedly argued as he combined antislavery agitation with political action. He rejected William

Lloyd Garrison's opposition to participation in the electoral process and supported Martin Van Buren, John P. Hale, John C. Frémont, Gerrit Smith (in 1860), and Abraham Lincoln (in 1864). With emancipation and the Union victory, Douglass became a loyal Republican who campaigned for Ulysses S. Grant, Rutherford B. Hayes, and their successors.

Stressing that Douglass was a leader and thinker, Blight admirably analyzes the millennial apocalyptic and providential sources in the reformer's jeremiad style. But too often the author attributes uniqueness and radicalism to Douglass, who actually shared ideas with some white and black abolitionists. (Comparing Douglass to Lewis Tappan, as if Tappan represented white abolitionist radicalism, is misleading.) Douglass, in fact, was no more forthright than the white "Secret Six" who supported John Brown and then after Harpers Ferry shirked from acknowledging Brown's insurrectionary intentions.

Because Douglass fundamentally was a platform agitator and journalist, responding to daily vicissitudes, Blight cannot portray him convincingly as an original and consistent thinker. He revealingly concedes: "To understand the political thought of Frederick Douglass, one must not be dismayed by his inconsistency. He was thoroughly human, complex, and contradictory, occasionally he was simply confused" (p. 58). There is too little in this book, however, about his human qualities. Minimally describing Douglass's sad relationship with his first wife, children, and brother, Blight avoids biographical and psychological issues. Yet the issue of Douglass's self-perception is too important to be ignored, even if the author strives to picture him as a thinker. Douglass's response to Lincoln, for example, reveals his view about himself in a white world and about the necessity of rejecting the widespread doctrine regarding the diversity and inequality of races. After their meeting in August 1863, Douglass observed about Lincoln: "He treated me as a man. He did not let me feel for a moment that there was any difference in the color of our skins." Here is a missed opportunity for Blight to link Douglass's opposition to the black colonization movement with his belief in interracial marriage.

Blight persuasively explains Douglass's postwar nationalism and support of American overseas expansion. Although deftly tracing Douglass's continuing fight for racial equality, civil rights, and the ballot, the author is silent, unfortunately, about his subject's failure to support women's suffrage.

Douglass continues to be a difficult subject for historians. Part of the problem is the sources: a very incomplete collection of personal letters but plentiful published articles and speeches by Douglass. Nevertheless, Blight has increased our knowledge of a mighty figure in American history while acknowledging an understandably heavy reliance on James M. McPherson's *Struggle for Equality* (1964). We still await a full-

scale Douglass biography, but this book is an indispensable step to that result.

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 Stony Brook*

KENNETH RADLEY. *Rebel Watchdog: The Confederate States Army Provost Guard*. Baton Rouge: Louisiana State University Press. 1989. Pp. xvii, 340. \$29.95.

Armies, in effect, are small societies, and, like larger societies, they need a police force to maintain law and order. A Confederate military police force, the provost or provost guard, made its appearance early in the Civil War as a means to help preserve military discipline and guard prisoners. During the war the provost took on additional functions. Eventually rebel provost guards were used to chase stragglers and deserters, collect and guard civil and military prisoners, safeguard (and sometimes destroy) Confederate property, control slaves, guard hospitals, enforce conscription, operate a system of internal passports designed to control both military and civilian travel, collect military intelligence, and pursue spies. In summary, the Confederate provost became a police force with far-reaching power over both soldiers and civilians.

As the provost guard's responsibilities grew, it came to play a more important role in Confederate life. It drew men away from front-line units, and its operations soon created resentment among both Confederate military and civilian personnel. Soldiers traveling on furlough, for example, lost valuable leave time waiting in a provost office to obtain the passports that were necessary for rail travel. Most civilians saw no reason why any military force should interfere with their travel, but they, too, were required to obtain passports from the provost. Many Confederates, therefore, thought of the provost as an example of military tyranny. Front-line troops tended to view provost guard members as rear-area soldiers who had found a safe haven that provided them with shelter from combat. The provost guard, therefore, had a mixed record. It both helped and hurt the Confederate cause. To the extent that it returned men to front-line duty or protected valuable government property, it helped; when it undermined support for the government, it hurt. All things considered, however, it strengthened the rebel military forces and to that degree helped prolong the life of the Confederacy.

In this book Kenneth Radley has produced a fine study of the Confederate provost guard. He emphasizes the guard's organization and evolution and the rapid expansion of its functions. Radley's research, almost all of it in published sources, is very impressive. It had to be since "provost guard" and "military police" are not items that are listed in many indexes. Radley, therefore, had to read through hundreds of sources seeking the few references to the provost that might have appeared in each. Research in newspapers and a

systematic study of individual service records in the National Archives would have added valuable details but probably would not have changed Radley's conclusions.

This study is not a book to read for blood-and-trumpet battle history, although at times provost troops were thrown into combat. It is a well-done bit of administrative history that adds much to our understanding of how the southern military effort functioned. Both historians working in other areas of Civil War military history and those studying various facets of civilian life in the wartime South can profit from Radley's work.

RICHARD M. MCMURRY
Decatur, Georgia

RALPH LOWELL ECKERT. *John Brown Gordon: Soldier, Southerner, American*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1989. Pp. xvii, 367. \$32.50.

John B. Gordon was a central figure in the evolution of the New South as well as in the evolution of New South historiography. Confederate war hero, a leader of the Ku Klux Klan during Reconstruction, and a spokesman for the New Departure (or Bourbon, depending on one's interpretation) Democrats, Gordon was, of course, a member of the "Bourbon Triumvirate" in Georgia. C. Vann Woodward in his epochal *Tom Watson* (1938) pointed to Gordon as a prime example of a new governing elite motivated by "the acquisitive zeal of the rising capitalists and industrialists whom he [Gordon] served." Woodward's interpretation of the New South has been widely challenged, but Gordon remains a key character in the debate. Clearly, an authoritative biography of Gordon would be an important addition to the literature; unfortunately, Ralph Lowell Eckert has not provided one.

Eckert's work is a nostalgic and admiring biography of a "southern spokesman and national statesman" (p. 296). The first third of the book retraces once again Gordon's "brilliant military record with the Army of Northern Virginia" (p. 2). Eckert then turns to the Gallant Gordon's efforts "to defend and promote the interests of his native South" (p. 2). During Reconstruction, Gordon was "at least titular head of the Georgia Ku Klux Klan" (p. 149), and he labored vigorously "to restore home rule to the South" (p. 181), to oppose the policies of the Radical Republican "enemies of the South" (p. 170), and to restore "traditional southern liberties" (p. 143). Naturally these endeavors earned Gordon a place in the U.S. Senate where "southerners could still rest assured that he would safeguard and promote their interests" (p. 157). Completely ignored are the facts that almost one-half of Georgia's citizens were black people and that they voted overwhelmingly against Gordon when he ran for governor on a white supremacy platform during Reconstruction. To Eckert, "the vast majority of Georgians stood behind their

beloved hero of Appomattox" (p. 224). After successfully defending the "South" from the machinations of carpetbaggers, Gordon progressed from being "a spokesman for and of the South" to becoming "a national statesman as well" (p. 163). Eckert refers to Gordon as a "Bourbon" and notes his devotion to "traditional southern social and political values," while portraying him as a champion of "the cause of industrialization" who endorsed "the capitalistic New South spirit" (pp. 240, 209).

I am left with one question: for whom was this book published? Libraries are burdened with works that address Gordon's military exploits, and indeed Allen P. Tankersley's *John B. Gordon* (1955) contains a more detailed account of Gordon's wartime experiences. Readers who wish to read a Dunning school interpretation of Reconstruction would do better by referring to C. Mildred Thompson's *Reconstruction in Georgia* (1915), which is far more informative than Eckert's work. Readers who wish to examine the path to national reconciliation might consult Paul H. Buck's *Road to Reunion* (1937), which treats the subject from the same perspective as does Eckert's book. Readers who wish to view a representative of the "capitalistic New South spirit" can learn far more by reading Woodward. Even readers who just want to find more detail about Gordon's life would be well advised to consult Tankersley's biography, which is more perceptive, less misleading, and more readable.

In a bibliographical essay, Eckert describes Tankersley's biography as "limited in scope, methodology, and treatment" and as "uncritical in its approach" (p. 355). These comments aptly describe Eckert's book.

NUMAN V. BARTLEY
University of Georgia

THOMAS A. BECNEL. *The Barrow Family and the Barataria and Lafourche Canal: The Transportation Revolution in Louisiana, 1829-1925*. Baton Rouge: Louisiana State University Press. 1989. Pp. xii, 202. \$27.50.

Thomas A. Becnel's book traces the family tree of the Barrows of Louisiana and the financial fortunes of the Barataria and Lafourche Canal that they controlled for so long. We learn how the state helped subsidize the canal's construction and improvement, how the Barrows' empire faltered in the Civil War and was revived thereafter. We learn what Robert Barrow, Jr.'s tax returns were and the frustrations of eradicating the water hyacinth. Finally, we watch as the canal prospered through the services of the Army Corps of Engineers, fended off rival railroads and canals, and was sold to the U.S. government in the early twentieth century.

Becnel can certainly gather facts. No one important appears without his or her ancestry and kinfolk detailed, much in the manner of an Icelandic saga. If a law was passed, a suit filed, or a financial statement issued that related to the Barataria and Lafourche

Canal, the book covers it. Flu, finances, family—all mingle together in seamless narrative.

Everything that Becnel says may be true. But to what end? The dust-jacket blurb boasts that the book "provides an opportunity to compare Louisiana's canal ventures with those of other states and reveals the extent of sugar and cotton planters' involvement in public transportation." The wording is precise, though possibly misleading. All comparisons must be left to the reader's own research. Aside from references to Carter Goodrich's survey, Becnel has nothing to say about other canals. He hardly reveals planters' involvement—unless the planters were Barrows or the involvement concerned the Barataria and Lafourche Canal. How did the canal change the larger economic fortunes of the parishes that it passed through? How did the ideology of development affect politics? What separated New South from Old in regard to economic values, and what did the canal say about the spirit and cost of either one? How did the canal's fortunes resemble those that other historians have analyzed in Michigan and Ohio? On none of these questions does Becnel more than touch. With the exception of a very few historians, cited lightly, there is no sense of larger context at all. Thus, Robert Wiebe's *Search for Order* (1967), a work constantly referred to and used to give this monograph a sense of context, appears to have told the author only that national markets were created in certain commodities and that there were "sweeping changes in the role of the national government, especially immediately after World War I" (p. 130). Did New Orleans newspapers, including the business press, discuss a canal significant to their interests? If so, Becnel has overlooked them. In 189 pages of text and ninety-six years of narrative, newspapers are cited only four times. Three citations are from the New Orleans *Picayune*, all of them referring to personal matters, and one is from the Houmas *Ceres*. Like the *Picayune* articles, apparently, it is a clipping from the Robert Ruffin Barry papers.

Whatever the real motivations were for writing this book, readers will find it hard not to make up their own: perhaps a desire to use the Barrow papers while they remained on indefinite loan at the author's place of employment, combined with an inability to think of any larger point that they might illustrate.

MARK WAHLGREN SUMMERS
University of Kentucky

CHARLES E. ORSER, JR. *The Material Basis of the Postbellum Tenant Plantation: Historical Archaeology in the South Carolina Piedmont*. Athens: University of Georgia Press. 1988. Pp. xvii, 322. \$35.00.

In this book Charles E. Orser, Jr., documents the metamorphosis after 1865 of life and labor on Millwood Plantation, a fourteen-thousand-acre tract straddling the Savannah River but located principally in Abbeville County, South Carolina. Established during

the 1830s by a relative of John C. Calhoun, Millwood was preserved more or less intact for the next century while successive generations of slave workers and their sharecropper descendants grew cotton at the behest of white landowners. Eventually acquired by the federal government, the land was flooded by dam construction in 1985 but not before Orser had completed an extensive excavation of plantation structures and artifacts. The results of that project compose the core of Orser's book, which also includes contextual chapters on Millwood's antebellum history, the growth of postbellum tenancy in South Carolina, and the living standards and geographical mobility of tenant farmers in the South as a whole. Discussion of the latter subjects incorporates an impressive range of evidence drawn from statistical surveys and rural community studies published before World War II.

Historians of the South will applaud Orser's efforts to ground archaeological analysis in modern research on the origins of sharecropping, despite the author's neglect of larger developmental processes so ably explored by Steven Hahn, David Carlton, and Barrington Moore, Jr. Familiarity with recent historiography is evident at several points, particularly in the argument that Millwood's settlement pattern reveals the use of "squad" organization as a transitional device between gang labor and family-based tenant farming. For the most part, however, conclusions derived from archaeological data are either ambiguous (for example, low correlations between artifacts and categories of land tenure) or unsurprising, as in the discovery that house size tended to vary with one's place on the "agricultural ladder."

In light of the book's professedly Marxian orientation, the lack of a more sophisticated approach to class relations requires comment. An exceedingly narrow, if not literal, definition of "historical materialism" pervades the book, closing off possibilities for thematic development and fostering intellectual compartmentalization. On the text's final page, for example, Orser argues that Millwood's material basis is best understood as the "inherent distinction between two groups of people who perceived the plantation as either home or real estate" (that is, tenants and landlords). The insight may be valid, but it is difficult to reconcile with the statistical discussion in chapter 4 of frequent transience or "shifting" among tenants. And, if sharecroppers did, in fact, regard temporary domiciles as "home," why does Orser assert on page nine that "landlords were interested in profits and plantation tenants were generally interested in getting away from the plantation"? Resolution of these issues would appear to lie beyond the methodological scope of the present volume.

CLARENCE L. MOHR
Tulane University

I. A. NEWBY. *Plain Folk in the New South: Social Change and Cultural Persistence, 1880–1915*. Baton Rouge: Louisiana State University Press. 1989. Pp. xiv, 588. \$35.00.

Forty-one years ago, in *Plain Folk of the Old South*, Frank L. Owsley promoted a revisionist view of antebellum southern society that included a large and relatively comfortable rural middle class that was not part of the region's plantation economy. I. A. Newby now urges a reconsideration of another generation of plain white folk who moved from the countryside into the cotton textile mills that mushroomed across the southern Piedmont in the late nineteenth and early twentieth centuries. This extensively researched book sympathetically examines the enormous changes these people made under the impact of industrialization and seeks to understand how they made the transition on their own terms. Yet the real significance of the work lies in Newby's determination to contest the stereotype of a helpless, ruthlessly exploited proletariat in the New South.

Focusing on one particular group of plain folk—the sharecropping families who constituted the bulk of the cotton mill work force in Georgia and the Carolinas—Newby thoroughly demonstrates both the diversity of their experiences and the persistence of their cultural values. Going from farm to mill was less disruptive than commonly assumed because the division between the two remained unclear and the movement from one to the other considerable. Families, especially those headed by women, generally went to the mills as a unit, an arrangement regarded by the folk and their employers as not only economically practical but socially and psychologically desirable as well. Once on the job, millhands found much that was familiar. The hours were longer compared to farming. But the work was less strenuous, almost as flexible, and moderately paced; the mills themselves were informal, sociable places; and one could escape or protest through absenteeism or "floating" among several mills. The paternalistic relationship between landlord and sharecropper was consciously duplicated in the mills by high-ranking officials and their employees. While mill villages were often drab, unhealthy places, Newby asserts that they also could be "vibrant, even exciting" (p. 259) communities where managers and mill workers alike assessed proposed innovations against the standard of folk traditions.

Although hardly isolated, mill society was a self-contained system. For the plain folk, religion was an expression of faith, not an instrument of control or a vehicle for reform. They believed that the purpose of schools was to teach literacy, that child labor grew out of both necessity and custom, and that reforms in either area endangered their incomes and their families. Viewing blacks as a threat to their jobs and racial prerogatives, the folk forced managers to minimize their presence in the mills. Their resistance to management demands did not lead to the organization of labor unions unless they were molded according to folk influences.

Newby's arguments will likely raise the hackles of those who are more critical of the New South's cotton mill system. The author grants that the textile industry

gradually became indistinguishable from other modern industries after 1915, yet others may wonder whether the transformation came sooner, since a majority of the book's positive descriptions of mill work come from the earliest years of southern industrial development. Some readers may be especially struck by Newby's characterization of the plain folk. He suggests that many were fatalistic, suffered feelings of dependency, guilt, and shame, and possessed a temperament that tended "toward psychological frustration, social irresponsibility, and compulsive violence" (p. 346). But rushing to a harsh judgment would be premature. As Owsley did four decades ago, Newby is challenging simple, facile explanations of the southern white working classes and showing how difficult, yet important, it is to understand them.

JOHN M. GLEN
Ball State University

STEPHEN WILLIAM FOSTER. *The Past Is Another Country: Representation, Historical Consciousness, and Resistance in the Blue Ridge*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xix, 262. \$25.00.

This book is a cultural analysis of identity and action in an Appalachian farming community in North Carolina. As an anthropological study of the community's culture and way of life, it focuses in particular on the fight against damming a local river to produce electric power for the surrounding region. Stephen William Foster analyzes the fight over the dam in terms of symbolic and rhetorical action by looking at the way positions were presented and framed by both sides. It examines the way in which a small community dealt with the encroachment of the outside world and sought to maintain a local identity in a world that devalued that identity.

History, to these embattled Appalachian farmers, is a matter of ancestry, place, and land. It is crucial to the formation of local identities. "As interaction derives ideologically from the definition of persons, so identity derives from history—personal history, family history, regional history, and finally history in a general sense" (p. 9). In the conflict over damming the river, history became part of the rhetoric of the battle. The river serves as a symbol of time and the historical continuity of the local way of life, a continuity threatened by the dam.

Ironically, as these Appalachian people turned to history in order to construct an identity against the outside, they lost control over that identity. They promoted a local symbolic identity through their crafts and music, which they marketed to the outside world. But customers imposed their own standards of taste, and the larger public adopted its own understandings of these objects. Baskets, quilts, oak butter churns, and other crafts were reinterpreted not as necessities produced by self-sufficient farmers but as symbols of a past

way of life, rooted in history but anachronistic in the present.

Theoretically, Foster focuses on the politics of representation and symbolic action, an exciting new approach within anthropology. I think the most interesting aspect of this ethnography for historians is its account of the way history itself becomes a cultural symbol, a resource in the struggle for autonomy and identity. That history serves as a charter for society, providing an account of the past that legitimates the present, is an old idea in anthropology. What is novel about this book is its application of this idea to a community undergoing pressure to change and its use of the idea in an explicitly political context.

But this book does not have to be as hard to read as it is. Interesting ideas are often buried under mind-numbing jargon. The ethnography itself is sketchy and uneven. Despite the appeal of the cultural analysis of the dam controversy, this dispute cries out for an examination of political economy as well. Foster's own account of the fourteen-year fight over the dam implies that all of the important decisions were taken at the federal level by courts, Congress, and the president. Yet his analysis focuses on a local festival that publicized crafts and music and a report written by local citizens. We have no indication of how much difference these events made. Indeed, Foster repeatedly refers to the "filigree" of power, suggesting by this choice of metaphor fragility rather than strength. This is a problem with cultural analysis in general, which tends to ignore forms of power that are not derived from representations and signs.

This book is valuable as a study of internal colonialism and of the way a small group struggled to maintain autonomy, create a positive identity, and guard their space as the larger world encroached upon them. As an exemplar of the possibilities of an anthropology of representation, consciousness, and resistance, it is both tantalizing and frustrating because of its opaque theoretical discussion interspersed with passages of great insight and clarity.

SALLY ENGLE MERRY
Wellesley College

CYNTHIA NEVERDON-MORTON. *Afro-American Women of the South and the Advancement of the Race, 1895–1925*. Knoxville: University of Tennessee Press. 1989. Pp. 272. \$34.95.

After attending a meeting of the National Association of Colored Women (NACW) in 1920, a white woman observed: "I saw women of education, culture and refinement. I had lived in the South all my life, but didn't know such as these lived in the land" (p. 228). These black women and their achievements have been similarly invisible in historians' accounts of progressive social reform, racial advancement, and the expansion of women's sphere. Cynthia Neverdon-Morton challenges this omission and argues that in the South black

women founded and headed most of the social service organizations that responded to the urgent needs of their race in the era of Booker T. Washington and W. E. B. Du Bois.

Cynthia Neverdon-Morton's study, which focuses principally on Hampton, Tuskegee, Nashville, Atlanta, and Baltimore, traces the growth of Afro-American women's educational opportunities and the social welfare organizations and clubs the first generation of educated black females established. Their education at schools as diverse as Fisk University and Tuskegee Institute imbued them with missionary zeal and the conviction that black women bore a special role in racial uplift. Rather than separating themselves from the uneducated and impoverished, they reached across class lines and built schools, orphanages, neighborhood centers, kindergartens, extension programs, and health services. In Atlanta, for example, the Neighborhood Center served as a powerful engine of social reform that provided day care, established the first black health center in the city, and effectively lobbied for better schools and higher teachers' salaries. The union became so successful that the National Urban League went to great lengths to force its incorporation into the League. The Atlanta women saw the organization as their own, refused to give up its autonomy, and effectively staved off the male-dominated League's overtures.

Women's projects often extended beyond the local level, and in some cases they transcended racial lines, thereby producing fragile but critical interracial cooperation. Yet black women generally did not have equal voices in the regional and national organizations. The Woman's Christian Temperance Union established separate branches. The Young Women's Christian Association maintained a highly discriminatory internal organization as well as segregated branches, and the National American Woman Suffrage Association and National Woman's Party restricted their Afro-American sisters to the fringe of the national suffrage movement despite black women's demands that suffragists recognize the need to ensure and protect the vote for all women. As the study points out, however, sexual advancement never took precedence over racial advancement. Within organizations committed to racial progress such as the National Association for the Advancement of Colored People and the Urban League, black women faced exclusion from most leadership positions. Yet the principal workers who achieved the groups' goals, argues Neverdon-Morton, were women.

The study's value would have been increased had the author analyzed black women's experiences in education, community building, and expansion of their sphere within the broader context of women's history. Important parallels between black and white women's search for new definitions of women's proper place merit exploration. White middle-class women reworked the "cult of true womanhood" in the late nineteenth century into a mandate for social action,

and recognition of the interplay of this idea between the races would have strengthened the study. Another important theme deserving exploration is black women's acceptance and adoption of "true womanhood" as a means of casting out the insidious, debasing image of the promiscuous Afro-American seductress. Paula Gidding's work, *When and Where I Enter: The Impact of Black Women on Race and Sex in America* (1984), would have served the author well in conceptualizing this study.

Finally, the editors did themselves a disservice by not being more attentive to matters of style. Despite these limitations, this study provides valuable information on Afro-American women's efforts as heroic leaders and indefatigable workers for their race in the South.

JUDY JOLLEY MOHAZ
Southern Methodist University

DAVID M. DEAN. *Breaking Trail: Hudson Stuck of Texas and Alaska*. Athens: Ohio University Press. 1988. Pp. xii, 344. \$29.95.

Hudson Stuck led an improbable life. An English Presbyterian, Stuck migrated, in 1885, to Texas in vague pursuit of fortune. In fairly short order, he took up another kind of destiny entirely, training as an Episcopalian minister at Sewanee's University of the South. Returning to Texas, he seemed to settle in as a prominent minister in Dallas, crusading against child labor and thereby making a number of his prosperous, industrial parishioners distinctly nervous.

Approaching forty, Stuck fully embraced improbability, volunteering to be a missionary in interior Alaska. In 1904, he set off to bring Episcopalianism to Alaskan natives and miners in an area "of some 250,000 square miles" (p. 76). Visiting villages by boat in the summer, Stuck also toured his domain in the winter, traveling hundreds of miles by dogsled, at temperatures in the unimaginable minuses.

Having placed himself in a new world, Stuck adapted magnificently. Beyond his winter village tours, he took part in the first successful, recorded ascent of the south peak of Denali (Mount McKinley) in 1913. He pursued, to the best of his understanding, the protection of the natives, arguing against full assimilation and for the continuation of traditional ways of making a living. He fought diseases and set up hospitals, overspending the mission budget when necessity seemed to justify it, and led in campaigns to prohibit alcohol and to shut down a cannery, at the mouth of the Yukon River, that greatly threatened native fishing. It was, genuinely, Stuck's hope that, in interior Alaska, white Americans had at long last found a territory that they did not want and that could therefore be saved as a home for the natives.

Campaigning for his causes or simply recording his life, Stuck wrote books, private diaries, and letters. Drawing on a treasure trove of previously "lost" diaries, David M. Dean has cornered the market in "Hudson Stuck studies," writing a definitive, detailed, and very

readable biography. Dealing with an eccentric man, Dean appropriately adopts a policy of full disclosure, exploring, for instance, Stuck's decidedly warm preference for the company of boys and correspondingly cool response to the company of most women (although he did reserve some passion—and fury—for missionary nurses who dissipated their usefulness by marrying local Alaskans).

Like the best biographies, this book is almost equally a portrait of the subject's setting. Getting a grasp on Stuck's life requires an equal grasp of interior Alaska in the early twentieth century, on mining towns riding an extraordinarily rapid boom and bust cycle, on native villages resisting, or succumbing to, the problems of alcohol and economic dependence.

This is an unforgettable book; the improbability of Stuck's life will not let the reader place the story in a comfortable file at the back of the mind. Is this story simply an Alaskan variation on "the white man's burden," or does the sheer distinctiveness of both Stuck and Alaska add up to something else entirely? One would have liked a little more help in interpretation from the author. A concluding chapter on the significance of Stuck in Alaskan—and even American—history would have allowed Dean to reap the full value of his biographical labors.

PATRICIA NELSON LIMERICK
University of Colorado

D. C. COLE. *The Chiricahua Apache, 1846–1876: From War to Reservation*. Albuquerque: University of New Mexico Press. 1988. Pp. vi, 219. \$32.50.

Over the past several decades, there has been a call for more American Indians to participate in the writing of tribal history. The arguments are compelling. Most tribal histories have been produced by non-Indians who tend to rely on traditional documentary sources, thereby ignoring the Indian perspective. By implication, at least, the conqueror mentality is thus perpetuated. A few Indian authors have recently undertaken successful tribal histories, and more are forthcoming. In this book, D. C. Cole, a Chiricahua Apache, presents the type of study that is called for, a wise blending of oral Indian history and traditional archival sources. Although the subject that Cole has chosen to detail represents only a fragment of Chiricahua history, his work adds substantially to earlier accounts of Apache history.

The Chiricahua Apache are famous in the annals of American history for producing such leaders as Cochise and Geronimo and for actively resisting the federal reservation program. Cole, however, does not dwell on the sensational accounts of warfare, raids, and glory too common to the topic. Rather, he explains the nature of these desert people, their culture and religious beliefs, and their views of the world. In particular, he develops and explains the concept of "power" (a supernatural force) as it influenced the everyday rela-

tionships between the Chiricahua and other peoples. Placed in this perspective, the differences in sexual roles and the ceremonial cycle blend into the chronology of events as the Chiricahua contended with a succession of invaders.

The historical narrative begins with the arrival of the Americans in the Southwest. By this time, the Chiricahua were fully committed to a way of life that included raids on the Mexican population. The Apache actually treated the Americans with some respect until ruthless individuals destroyed any sense of trust. For the U.S. government, the solution to an increasingly violent situation was to confine the Apache on reservations. In this case, the efforts of Thomas Jeffords and Cochise ultimately resulted in a favorable arrangement for the Chiricahua—namely, the establishment of a reservation in their own homeland of southeast Arizona. Cole is at his best describing life on the short-lived Chiricahua reserve (1872–76) and how a promising experiment turned sour in the face of white opposition and the inability of the Indians to make the cultural adjustment demanded by the government. As the author correctly concludes, the parties involved approached "the situation with their minds already filled with the impediments of past occurrences" (p. 169). The inevitable result was a new removal and another round of violence.

The author is to be commended for producing a readable book that is not only in accord with recorded history but also enriched with the element of tribal tradition and custom. The two are skillfully blended together in a very satisfactory fashion. The unsuccessful four-year experiment with a reservation geared more to Indian than to white interests becomes even more understandable when placed in an ethnohistorical perspective.

ROBERT A. TRENNERT
Arizona State University

ROBERT S. MCPHERSON. *The Northern Navajo Frontier, 1860–1900: Expansion through Adversity*. Albuquerque: University of New Mexico Press. 1988. Pp. viii, 133. \$22.50.

In the late nineteenth century, at a time when other western tribes suffered major reductions in their land-holdings, the Navajo witnessed a significant increase in their already sizable reservation. Most of the growth came along the reservation's northern boundary, and Robert S. McPherson examines the history of this Indian success story. McPherson offers the provocative thesis that the Navajo themselves deserve credit for increasing their tribal domain. According to the author, throughout the late 1800s, the Indians pursued a conscious "aggressive defensive policy" (p. 95) against other interests competing for hegemony in the frontier area. Using this strategy the Diné achieved a remarkable success, while for other tribes allotment was the order of the day.

McPherson describes several ways by which the Navajo expanded their influence over adjacent territory. One technique that they used quite effectively was to flood nonreservation grazing lands with large herds of sheep, thereby undermining the ranching efforts of Anglo-American cattlemen. Furthermore, when miners encroached into the area, occasional acts of violence occurred, intimidating even adventurous whites. These conflicts, however, were never allowed to swell to such a dimension as to require the intervention of the military—the Indians were too smart for that, argues McPherson. Finally, at the century's end, the Navajo embraced a government-sponsored farming program in the valley of the San Juan River, the region's principal watercourse. This project placed a large number of Navajo north of the reservation with the blessing of the federal government. Extensive grazing, a fear of violence, a growing Navajo population, and off-reservation farming all combined to force the government to redraw reservation boundary lines, recognizing that the Navajo had gained de facto control of vast holdings beyond the reservation.

This book is an important addition to a number of recent scholarly publications on the Navajo, and McPherson deserves to be commended for a path-breaking effort. Nevertheless, some serious methodological problems exist. The greatest shortcoming is the narrative gap between the author's thesis and the demonstrative evidence required to prove his theory. McPherson rarely mentions tribal leaders, and much more attention needs to be paid to their views if historians are to believe that the Navajo themselves played a dominant role in expanding the reservation. At the same time, a detailed examination of all government correspondence on the subject must be made in order to determine the federal government's reasons for permitting the additions. As it now stands, acts that the author interprets as part of a conscious Indian endeavor to increase reservation lands can just as easily be construed as isolated incidents. McPherson is on solid ground when he urges scholars to focus on the Indians as key actors in their own history, but the author either chose to ignore tribal leaders throughout his work or was unable to determine their actions. It is my feeling that extensive research into all exchanges between prominent Navajos and governmental agents will confirm or deny the book's main premise.

Despite these criticisms, McPherson's book is valuable, and his approach offers the possibility of obtaining the Indian viewpoint. This constitutes a major achievement and should not be discounted. Because a good general history of the Navajo between the years 1868 and 1912 does not exist, historians can only speculate about McPherson's ideas. Did the Navajo boldly extend their own reservation while most other tribes suffered losses as a result of federal Indian policy? Was this a case of an American Indian "aggressive defensive policy"? These important questions re-

main unanswered, but credit McPherson for asking them.

GERALD THOMPSON
University of Toledo

WILLIAM E. UNRAU. *Mixed-Bloods and Tribal Dissolution: Charles Curtis and the Quest for Indian Identity*. Lawrence: University Press of Kansas. 1989. Pp. xii, 242. \$27.50.

In this intriguing biographical study of "Indian Charley" Curtis's career, William E. Unrau sheds new light on the multiplicity of motivations surrounding the breakup and allotment of Indian reservations in the trans-Mississippi West.

Charles Curtis, a mixed-blood Kansa-Kaw attorney and congressman from Kansas, became a leading figure in the development of American Indian policy in the late nineteenth and early twentieth centuries. The Curtis Act of 1898, which essentially provided for the dissolution of tribal courts and governments within the Indian Territory and paved the way for Oklahoma statehood, has been called the most influential piece of Indian legislation between 1887 and 1934. A political power within the Republican party and on Capitol Hill for over twenty-five years, Curtis achieved his greatest national prominence as vice-president of the United States during Herbert Hoover's administration.

Curtis was Indian by virtue of one-eighth blood quantum from his mother's line; he was also the beneficiary of a substantial estate from his maternal grandmother—land acquired in the Kansa treaty of 1825—which contained valuable property near the center of what was to become Topeka, Kansas. Nevertheless, Curtis was reared and educated by his paternal grandparents well away from the reservation. In fact, Curtis's mixed-blood grandmother advised him to separate from the tribe and "make somebody" of himself (p. 93). Accordingly, he adopted most of the economic and political values of frontier whites. At the same time, Curtis aggressively promoted a leadership role for mixed-bloods in fostering the "progressive" federal policies of his day. Unrau writes, "Being an Indian, for Charles Curtis, was less a matter of blood than of fusion of Indian and white into a cultural composite that was committed to private property as the most effective means of fulfilling the assimilationist dream" (p. 5).

Unrau's effort to explore Curtis's role as the leading mixed-blood Indian spokesman in the nation at the turn of the century is hampered by an inexplicable paucity, for such a public figure, of manuscripts, correspondence, speeches, and reminiscences. As a consequence, although the work is meticulously researched and quite well written, one comes away feeling that only occasional glimpses of Curtis the person come through. The author has presented a balanced assessment of Curtis's promotion of legislation that, in retrospect, appears more self-serving than of service to the public or Indian interests. For example, Curtis's efforts to have himself reinstated on the Kaw tribal roll

by 1902 in time to acquire 160-acre allotments for himself and his children, and later having himself designated an "incompetent Indian" to avoid tax liability for the increasingly valuable property, smack of callously exploiting one's Indian heritage.

Although the general history of Indian reservation breakup and allotment is well known, historical studies have lacked a unique perspective on Curtis. It now appears that Charles Curtis's position in American Indian history will have to be significantly reassessed by scholars as a result of Unrau's work.

HARRY A. KERSEY, JR.
Florida Atlantic University

CARLOS A. SCHWANTES. *The Pacific Northwest: An Interpretive History*. Lincoln: University of Nebraska Press. 1989. Pp. xxi, 427. Cloth \$32.95, paper \$15.95.

The central thesis of Carlos A. Schwantes's book is that most of the history of the Pacific Northwest can be framed in a locational context and explained as the consequence of the region's relative position vis-à-vis the remainder of the United States. Schwantes defines the Pacific Northwest as "hinterland," a region that is tributary to the dominant regions of the country and that tends to exist primarily as a supplier of raw materials to those regions. Although the hinterland argument, which runs throughout the book, is a reasonably persuasive one, it provokes the author to skirt the margins of geographical determinism. There can be no question that, in modern geographical terms, the Pacific Northwest is a peripheral region that bears a relationship to the core of the United States that is essentially the same as that of core-periphery relationships elsewhere among developed regions. To pin an "interpretive history" on a locational condition that can be so easily conceded seems simplistic and may have caused Schwantes to diminish other factors in the creation and development of a primary region of the United States.

Given this difficulty with the work's central theme, the study is nevertheless an intriguing and complete examination of the Pacific Northwest defined as the modern-day states of Idaho, Oregon, and Washington. The author first develops a sense of place for the region and then proceeds to discuss what he views as the five major periods of Northwestern history: the early emergence as a maritime frontier of European colonial powers and the young republic; the pioneering agricultural development and the subsequent creation of the region's social and cultural patterns; the transition from frontier to urban-industrial society that accompanied the introduction of the railroad and the forging of firm transportation links to other regions; the period of social unrest and conflict that emerged during the years prior to World War II; and the later period of "coming of age" in which the region has attempted to balance its awakening environmental consciousness with a desire to become something other

than a hinterland. Each of the sections of the book that covers one of these periods is begun with a "profile" of a key player on the historical stage during the period; this is a stratagem that works well in giving the book a sense of personalities at work as well as the more abstract forces of locational theory. Although this period organization is logical, it does produce some disjunctive feelings for the reader as it becomes less and less clear whether the author is producing a historical geography, an economic history, or a political history of the region.

The work is attractively packaged with many excellent maps, tables, and other illustrations. The serious reader will regret the paucity of reference notes, but the work is a significant one for its organizational qualities and would certainly serve as an excellent undergraduate text for courses in Pacific Northwest history.

JOHN L. ALLEN
University of Connecticut

DAVID M. EMMONS. *The Butte Irish: Class and Ethnicity in an American Mining Town, 1875-1925*. (Statue of Liberty-Ellis Island Centennial Series.) Urbana: University of Illinois Press. 1989. Pp. xiii, 443. \$25.95.

David M. Emmons has given us a rich portrait of the evolution of the Irish immigrant community of Butte, Montana. Copper, a metal vital in the accelerated industrialization after the Civil War, transformed Butte from a raw gold and silver mining camp into a working-class metropolis similar in ways to the urban areas of the settled Midwest and East. But Butte was special in two ways. First, it was described as the "Gibraltar" of labor unionism, a community where trade unions had organized every occupation and had maintained a closed shop. Second, it was an urban center in which the immigrant and first-generation Irish formed the numerical majority in the city and surrounding Silver Bow County. Thus, Emmons allows us to explore an immigrant community in which the inhabitants had acquired apparent economic clout and political dominance, and this experience is implicitly contrasted with that of immigrants in other communities where the newcomers were an oppressed and sorely exploited minority.

The Irish poured into Butte, pushed by economic deprivation and pulled by the promise of a "fair living," that is, good wages and steady work, in the copper mines. And the leading copper baron, Marcus Daly, was also Irish, decidedly partial to the employment of his ethnic brethren, and not, most important, unfriendly to unionism. Daly thus was important to the development of Butte as an Irish- and union-dominated town. But the stability of the Irish community generated a conservatism that stood in sharp contrast to the militance of western organized labor and, especially, the Western Federation of Miners.

Relying on a treasure trove of surviving records of Irish associations, Catholic churches, and census tran-

scripts, Emmons is particularly strong in detailing the endemic tensions between class and ethnic identity. The vast majority of Irish were workers, and most worked in the mines. They debated whether they should make common cause with fellow workers or give their primary loyalty to their nationality. Emmons demonstrates that the answer to this question was largely determined by generational position. The older, settled Irish miners with families, men who might have known Marcus Daly or who revered his memory, identified strongly with the interclass communities of church and all Irish men, both of which encouraged a transcendent unity above the vicissitudes and potential violence of class conflict. Younger Irish immigrants, usually single men and transients, were less attracted to Irish fraternal and nationalist organizations and neutral or often hostile to the Catholic church. These men were the most likely and willing recruits for the secular radical ideologies offered by the socialists and the Industrial Workers of the World (IWW). It was this intergenerational tension among the Irish, Emmons persuasively argues, that was a primal cause in the destruction of the Butte Miners' Union (BMU) in June 1914 and the imposition of a twenty-year open shop in the Butte mines.

The BMU was the crown jewel, the "Local No. 1," of the Western Federation of Miners. Settled, conservative, and somewhat xenophobic, the first generation of Irish miners made the union a vehicle for the defense of the perceived privileges of the Irish "labor aristocracy" rather than an organization for the defense of all miners of all ethnic backgrounds. As the work force became more ethnically diverse and as machine technology and the division of labor replaced the relatively skilled old-time miners, the BMU became increasingly irrelevant to new men. Restless and unassimilated, these men were open to the appeals of radical industrial unionism and willing to make common cause with the "Bohunk" and Finnish miners. Together, they destroyed the BMU and formed the base for several unsuccessful attempts by the Wobblies and other labor radicals to establish a new miners' unionism in Butte. Consequently, Emmons's research is a welcome addition to the history of labor unionism in the western mines and the history of the Western Federation of Miners in particular. Emmons's work, when read in conjunction with Vernon Jensen's *Heritage of Conflict* (1950) and my *Gibraltar: Socialism and Labor in Butte, Montana* (1988), gives us the most complete picture we are likely to get of the demise of the BMU, one of the most tragic episodes in American labor history. Emmons has made a significant contribution to the growing body of work dealing with the history of immigrant workers in the United States.

JERRY W. CALVERT
Montana State University

DAVID WARD. *Poverty, Ethnicity, and the American City, 1840-1925: Changing Conceptions of the Slum and the*

Ghetto. (Cambridge Studies in Historical Geography, number 13.) New York: Cambridge University Press. 1989. Pp. xiv, 263. Cloth \$39.50, paper \$13.95.

The key word in the title of David Ward's book is "conceptions." The subjects are not poverty and ethnicity in American cities but five divergent interpretations of the residents and the environment of America's slums and ghettos. The first three "conceptions" were formulated between 1840 and 1925; the last two were "reformulated" (p. vi) later.

Between 1840 and 1875 America's slums were conceived as parts of cities from which the rich had departed. Left behind were poor persons exposed to the worst of their lot. Slum inhabitants were classified morally, and most efforts to aid them were through moral uplift by evangelical Protestants. Although the slum environment was dirty, congested, and led to immorality, little was done to improve it.

From 1875 to 1900 the second interpreters of the slums saw an inner city filled with a lower order of people, some of whom were immigrants. Philanthropists tried to be scientific but continued to view slum inhabitants as either moral or immoral. Slum environs were still dangerous because they were dirty and congested. Sanitary improvements made by cities were successful, but efforts to reduce congestion were not.

Progressives reinterpreted the slum or ghetto between 1900 and 1925 as a place where principally immigrant residents suffered from "low or uncertain incomes" (p. 97). Poverty was the result of unemployment, and housing in slums was bad because people could not afford any better. Progressives made numerous studies that disproved the previous theories that immorality caused poverty, but they did not produce many policy changes.

The fourth interpretation of the slum was reformulated by the Chicago School of sociologists, who argued that heavy migration to cities led to conflict between tradition-minded first-generation immigrants and their children. The ghetto was a natural environment that met the needs of immigrants, but, as they assimilated, they moved out of it and scattered.

Since 1960 a fifth conception has challenged the idea of the ghetto: no ethnic groups were ever confined to one part of a city but instead dwelt in "discrete communities" (p. 182) in inner cities. First-generation immigrants did not stick to tradition but created new ethnic networks in America to help themselves and their children. When they moved out of the city, voluntary associations held them together; they did not entirely assimilate.

Ward presents a fascinating, complex view of how America's slums have been variously interpreted. These interpretations are principally of New York City, however, which is peculiar in a book emphasizing geography. Moreover, the author's first two conceptions reflect similar attitudes toward and methods of dealing with slums and might better be defined as one conception. These two objections aside, this closely

argued book is valuable because it represents a new, unique, and interpretive approach to urban historical geography.

PRISCILLA FERGUSON CLEMENT
*Pennsylvania State University,
 Delaware County Campus*

CRAIG H. ROELL. *The Piano in America, 1890–1940*. Chapel Hill: University of North Carolina Press. 1989. Pp. xix, 396. \$35.95.

In this book, Craig H. Roell convinces the reader of the importance of music and the piano to the change from a culture of production to a culture of consumption. The author competently outlines the transition and weaves together a wonderful array of sources, including the sales records of piano manufacturers, piano lesson books, and material on the piano industry collected by the National Recovery Administration and the Works Progress Administration.

Roell examines the place of the piano in the Victorian home, then carries the story through the Great Depression and briefly into the post–World War II era, where he views the piano as a Victorian relic in a consumer culture. The book presents the rise, decline, and resurgence of piano sales in the early twentieth century. Roell asks why the sale of pianos fell off in the prosperous 1920s and then picked up in the depressed 1930s.

In Victorian society, students produced music only after practice and hard work, so the introduction of the player piano was a crucial event. The player piano, emblematic of the new culture of consumption, allowed people to consume music passively and thus hurt sales of ordinary pianos while paving the way for the success of the phonograph and radio. Roell shows, however, that the player piano was the savior of the piano industry as well as its scourge. The industry portrayed player pianos as easy to play and fun rather than as a moral influence mastered only with the investment of long hours. Thus, piano manufacturers worked to commercialize music and increase the piano's popularity in the 1930s. Roell believes that changes in the public perception of music and pianos at once mirrored and contributed to the shift from a production to a consumption ethic.

The book suffers from a lack of precision in words and concepts. For example, Roell speaks of piano playing as productive work. For Victorian women, playing the piano was work in the sense that it contributed to the refinement of the home, and playing the piano was an active rather than a passive activity. But the author ignores the economic history of the word "productive." Even more problematic is Roell's concept of democracy. Roell shows that piano manufacturers advertised a musical democracy that made "music a consumable object available to all without the need of painstaking cultivation" (p. 32). Piano manufacturers did sponsor class lessons in the public schools, but the

purchase of a piano was possible for only a relatively few people. A commercial democracy is a sham, accessible only to those who have the money to participate. Roell adopts the word "democracy" without critically examining its use to describe both a situation where only those in the middle-class bought new pianos and an ideology based on a subservient female musician kept in the home.

Roell describes the piano as a gendered instrument, but the book would have profited from a more complex rendering of the difficult concepts of "home" and of women's work and how these changed over time. When Roell discusses the piano as being a female instrument and the implications for men, his explanation is better than when he discusses what this meant for women. Similarly, his analysis of the differences between the Victorian home and the consumer home is better than his examination of the many similarities between the two. Roell sets the Victorian and the consumer cultures in opposition, despite the fact that the Victorian era exhibited many elements of a culture of consumption. Industrialization and an emphasis on consumption emerged earlier and more slowly than Roell's somewhat rigid framework suggests, and these inaccuracies confuse the picture he presents.

Yet Roell details the construction of a consumer society and thus helps fill in the sometimes sketchy outline of a culture of consumption. His work is strongest when he writes about the piano industry, but Roell does more than trace the business history of the piano. He places the piano in a historical context and begins the story of how music became a consumer good.

SUSAN SMULYAN
Brown University

BRIAN MCALLISTER LINN. *The U.S. Army and Counterinsurgency in the Philippine War, 1899–1902*. Chapel Hill: University of North Carolina Press. 1989. Pp. xiii, 258. \$34.95.

Brian McAllister Linn's study, as viewed from the narrow parameters that he sets forth, is developed fully. Linn acknowledges that he is not a scholar of the Philippines. Yet he abandons a current term, "the Philippine-American war," favored by scholars of the American era in the Philippines. The conflict was between the government of Emilio Aguinaldo and the U.S. government. Linn is correct in rejecting the nomenclature "the Philippine insurrection" and "Filipino-American war." He prefers, simply, "the Philippine war." Americans know that the Founding Fathers in July 1776 represented only one-third of the people living in the thirteen colonies. But Americans have problems with the validity of Aguinaldo's proclamation of independence from Spain on June 12, 1898. Americans know the Articles of Confederation of 1777 did not enjoy the support of everyone living in the United States. Yet the Philippine Republic established under

the constitution of January 20, 1899 (with its fourteen titles and 101 articles) is faulted because it did not have the support of everyone living in the archipelago. If we agree that the leading elements of Philippine society, albeit principally from Luzon and neighboring islands, established a republic under a benevolent and generous constitution, just as did leading radical elements in the thirteen colonies, then the conflict between the U.S. and Philippine forces takes on a different hue. The U.S. Army, the Treaty of Paris that exchanged Spanish sovereignty for American control notwithstanding, was a tool of American imperialism. The fact that the Philippine Republic's army was reduced to guerrilla units so soon after the war began and that there was withdrawal of support from the republic by many rich and powerful Filipino families after the conflict started does not alter the premise that the U.S. Army crushed an attempt by people to establish a nation-state.

It appears that Linn wants to avoid dealing with why the American army found itself in Manila. After all, the professed reason for the Spanish-American War was Cuba's freedom. Linn's Philippine war acknowledges the conflict: the American military was in the Philippines, and it had a professional job to do. There have been many books over the last ninety years concerning that war. In most studies the U.S. military has been seen in varying degrees as reluctant citizen-soldiers to merciless brutes terrorizing the Filipino people. The generals commanding the U.S. forces appear either as brilliant or as bunglers who prolonged the conflict. Linn's monograph centers on Luzon and only four military districts on that island, two in the north and two in the south. He examines in considerable detail the scope and character of Philippine resistance and the U.S. response. He shows that there was no uniform plan emanating from the high command other than bringing the struggle to an end with a minimum of bloodshed. He gives his readers through extrapolation a larger understanding of the complexities facing Americans throughout the archipelago as they brought about U.S. control. Linn would have enhanced his study, in view of its title, if he would have included some other regions of the Philippines. It would have been useful if he had said something about the U.S. Army in the years between 1865 and 1898. Many of the American officers had firsthand experience fighting Indians and dealing with their recalcitrance. Did this prior experience allow these officers to deal forcefully with poorly armed combatants? What we need now is a well-researched monograph on the Philippine army and citizenry in their struggle against the U.S. Army.

MICHAEL PAUL ONORATO
California State University,
Fullerton

ROBERT C. FULLER. *Alternative Medicine and American Religious Life*. New York: Oxford University Press. 1989. Pp. viii, 164. \$19.95.

One hallmark of postmodern medical culture is the flourishing of holistic healing systems alongside highly reductionist, scientific medicine. The unmistakable surge of interest in alternative medicine since the 1960s has prompted a number of historians to search the past for patterns of behavior and belief in unorthodox healing that may shed light on the present, and this current in medical historiography has produced an increasingly sophisticated literature on the cultural meaning of unorthodox healing. In this volume as well, Robert C. Fuller explicitly uses the present as a springboard to the past. Rather than focusing on a single alternative medical movement, however, he traces what he convincingly displays as a persistent theme in unorthodox medicine in America, that is, its association with unorthodox, unchurched religion.

Fuller roots his story in the leading nineteenth-century unorthodox medical sects: Thomsonianism, homeopathy, and hydropathy. Ordinarily such movements were not overtly religious, but Fuller shows that they resonated with theological symbols and embodied a partly metaphysical view of reality. Emanuel Swedenborg and Anton Mesmer especially influenced alternative medical belief in America and helped forge what Fuller terms a "metaphysical connection" between physical healing and higher spiritual agencies. It was out of this metaphysical milieu characteristic of American religious culture that chiropractic and osteopathic medicine emerged late in the century. As Fuller shows, the American founders of both systems infused them with metaphors and images drawn from earlier mesmeric and spiritualist sources.

The alternative healing systems that proliferated from the 1960s on continued to serve as a vehicle for an unchurched strain of American religious thought. Fuller persuasively reveals the metaphysical and sometimes openly religious overtones to the systems conventionally grouped as holistic medicine. A metaphysical interpretation of reality is proselytized by the language and underlying world view of such diverse healing practices as psychic healing, Therapeutic Touch, acupuncture, spirit channeling, and New Age crystal healing. Such movements, Fuller asserts, must be taken seriously as expressions of an enduring mode of American spirituality, one that addresses the spiritual needs of many Americans in a secular age skeptical of established religious authority. Equally, the appeal of the eclectic therapeutics that these metaphysical assumptions sustain should be seen as one response to the spiritual bankruptcy of orthodox medicine.

It is no criticism to point out that, in concentrating on the religious associations of unorthodox healing, Fuller gives little attention to gender, class, and politics, factors that other historians have found particularly telling in understanding the animus and appeal of alternative medicine. His decision not to dwell on the healing activities of credal religions, even Seventh-Day Adventism or Christian Science, is consistent with his objectives. Less satisfying is his failure to appraise the religious texture or holistic impulses in established

medicine or to consider adequately what defined the medical orthodoxy to which other systems were alternatives. Nevertheless, this fascinating volume is a thoughtful and enlightening contribution to the history of American medical and spiritual life. It should stimulate interest in long-neglected themes, such as the important link between Swedenborgianism and homeopathy, and provide a suggestive map for exploring the thriving alternative medical enterprise of recent decades.

JOHN HARLEY WARNER
Yale University

DONALD K. GORRELL. *The Age of Social Responsibility: The Social Gospel in the Progressive Era, 1900–1920*. Macon, Ga.: Mercer University Press. 1988. Pp. x, 354. \$39.95.

The first two decades of the twentieth century witnessed an explosion of human energy with few parallels in American history. The flurry of idealism and activity that made up the Progressive movement, though brief in retrospect, left an indelible imprint on American society. Coexisting and interacting with the prophets of Progressivism were the Social Gospelers, mainline Protestants who sought to engender a dimension of social concern into their denominations. Donald K. Gorrell's book provides scholars with the first in-depth look into the denominational networks that sponsored the Social Gospel. His study covers the movement through years that others have ignored, painstakingly tracing the evolution from small visionary faith preached by a few nationally known preachers in 1900 to major plank in the mainline denominations by 1910. Coordinated through the Commission on the Church and Social Service of the Federal Council of Churches, the movement prospered and gained increased attention during the decade leading up to World War I.

Gorrell identifies the five most important Protestant organizations behind this push as Congregational, Episcopal, Methodist, northern Baptist, and Presbyterian. Except for noting some dissension on the part of southern Presbyterians, he does not address the question of regional commitment or conflict. This is an institutional history of the movement as manifested by the major denominations who were a part of the Federal Council of Churches. In that sense, it is clearly a history of church leadership, not a history of the commitments of the American worshiper.

Historians of the Social Gospel have generally noted the importance of the war in muting the success of the movement. Gorrell argues that the war itself actually heightened the Social Gospel vision by turning a national agenda into a potentially world-wide program. Yet the shift in the mood of the country that followed the conflict did significantly alter Social Gospel hopes. Paranoia about socialism, weariness with the nonstop penchant for change, and internal dissension all played a role in the demise of the idealism brought by the war.

Still, Gorrell maintains, the Social Gospel survived. Although adapted to the conservatism of the 1920s, a social message remained on denominational agendas as a result of the bureaucracy built during the previous two decades.

Gorrell's analysis avoids speculation on the specific successes or failures of the Social Gospel in American society and emphasizes the overall victory in achieving a sense of social responsibility within the mainline denominations. He plays down the traditional emphasis on conflict between individual and social salvation by correctly noting that most Social Gospelers retained an interest in both and sought to fuse the evangelistic appeal with their concern for restructuring society. The book offers a balanced, well-researched, and well-thought-out portrait of a period of rapid change in American church life. Effectively, Gorrell challenges the reader to recognize the contribution of church officials without deifying their efforts or lamenting their failures.

JAMES R. GOFF, JR.
Appalachian State University

JOHN MODELL. *Into One's Own: From Youth to Adulthood in the United States, 1920–1975*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xiv, 414. \$40.00.

John Modell's study provides a blueprint for the history of family formation in the United States from 1920 to 1975. The subject of the book is "how people grow up," the way in which "the push 'into one's own' was repeatedly revised over a half-century" from 1920 to 1975 (p. 7). Modell uses the life course approach to examine this subject by looking closely at the timing and sequencing in successive birth cohorts of a series of life transitions that mark the path "from youth to adulthood" in American culture. The transitions that Modell studies are leaving school, military service for men, beginning work, premarital coitus, marriage, parenthood, and divorce.

The life course perspective is fundamentally demographic and quantitative, and these features direct the discussion away from the level of the individual. Modell is interested, however, in the relationship of individual experience to aggregate phenomena and contends that the life course is a series of individual decisions that are structured rather than determined by external factors (p. 25). Moreover, he links the levels of individual, birth cohort, and social context by "fitting individual transitions into the socially constructed life course" and then in turn fitting "this life course into the material and institutional imperatives of the day as they impinged on individuals" (p. 25). Modell argues that the stage of youth has assumed greater importance in the twentieth century, that the youthful life course has been quite malleable over this period, and that youths themselves have increasingly articulated their own views of how they wish to grow up (pp. 18, 26).

Modell's picture of aggregate trends in the youthful life course makes an enormous contribution to our understanding of family formation in the twentieth century. He divides the years from 1920 to 1975 into five chronological periods and for each focuses on progressive stages in the sequence of transitions: the 1920s are organized around the new institution of dating; the discussion of the depression years centers on engagement; the section on World War II focuses on military service and marriage; the baby boom logically centers on parenthood; and the period from 1960 to 1975 highlights the advent of a new sexuality. Fortunately, Modell combines his often-cumbersome descriptions of quantitative trends with fascinating material on the experience of youth. For example, in the chapter on the 1920s, data on aggregate trends, such as the extension of schooling later into the life course but entry into the labor market at earlier ages, are linked to lively depictions of the emergence of the peer-regulated practice of dating and the eroticization of the path to marriage. His chapters on World War II and the baby boom present data on high marriage rates, early marriage, and prompt parenthood and set these in the context of rich material on the sexual attitudes of soldiers and their brides, as well as descriptions of the burgeoning high school youth culture and the advent of "going steady."

Modell is interested in differences in life course experiences based on gender, race, and social class. Although he presents much useful data on comparisons by race, the account is, by his own admission, most successful with regard to gender. Modell is quite sensitive in using statistics to interpret highly nuanced individual and social experience, but such interpretations can be tricky. For example, although quantitative measures demonstrate Modell's claim that, "overall, many of women's life course patterns have come more nearly to approximate men's" (p. 36), his later conclusion that, "from labor force participation to understandings of sexuality, American women have become more like men than they were" raises complicated questions about gender (p. 328). Women may have behaved like men, but they may have done so for entirely different reasons and while holding a radically different set of values. One needs to look carefully, as Modell generally does, at the content of this shift in behavior and these new gender roles.

Finally, the title of the book is a misnomer: the book is about family formation and not the transition from youth to adulthood. Modell's appropriate focus on demographic transitions requires him to emphasize marriage and parenthood, but he is surprisingly uncritical of the conflation of family formation with adulthood. He offers no discussion of the changing content of adulthood as a stage of individual psychological and emotional development, as opposed to attainments in social status, and no definition of the meaning of "into one's own." Most important, until the chapter on the 1970s when family formation and adulthood are finally disentangled, there is no discus-

sion of the transition to adulthood for the significant percentage in the population who remained outside of the family and never married. These are crucial questions if one is examining the transition to adulthood, though less so if one is studying family formation. On the important topic of the path to family formation, this book is a pathbreaking contribution to U.S. family history and social history in the twentieth century.

MARGO HORN
Stanford University

CARY D. WINTZ. *Black Culture and the Harlem Renaissance*. Houston: Rice University Press. 1988. Pp. 277. \$27.50.

With good reason, the Harlem Renaissance has been the most discussed and analyzed episode in twentieth-century African-American history. Since 1970, three major books have appeared, each comprehensive and scholarly. There have been several anthologies, substantial biographies of principal figures, and numerous scholarly articles. The basic shape of the period is, by now, well understood; the questions and lines of argument are familiar to us all.

Cary D. Wintz covers the same ground, adding little that is new. The primary distinction of his research is his use of the Amistad Collection at Tulane University and the Harry Ransom Humanities Research Center at the University of Texas. These collections have provided additional details on relationships between some of the Harlem figures and publishers, particularly the Knopfs. They also contain material on a short-lived magazine, *Contempo*, that, although southern (Chapel Hill), opened its pages to black writers. Discussion of this literary magazine and others such as *Palms: A Magazine of Poetry* and *Contemporary Verse* is too brief, yet it is the most original part of Wintz's work.

Aside from details, readers familiar with the scholarly writing on the Harlem Renaissance will find that Wintz adds nothing new in fact or interpretation. The result is a rather perverse example of scholarly discourse, in which the author dutifully acknowledges—in his "Note on Sources" and occasional footnotes—the presence of others who have written on the same subject and have addressed the same questions. Yet he never in the text engages them or the issues critically. It is as if Wintz has accepted all that has been written and merely wants to add his word as an increment.

NATHAN IRVIN HUGGINS†
Harvard University

JEFFREY S. GUROCK. *The Men and Women of Yeshiva: Higher Education, Orthodoxy, and American Judaism*. New York: Columbia University Press. 1988. Pp. xiii, 302. \$35.00.

Centennial celebrations of institutions as well as nations naturally encourage a penchant to look backward. The impulse to review the past can be both a boon and a bane for historians who discover their special skills in

demand. As historians we welcome the public's desire to know more about the past, but we also fear that this reawakened interest in what was may set the agenda of historical inquiry. Jeffrey S. Gurock, known for his social history of the Jews of Harlem, has negotiated admirably the pitfalls of writing an institutional history to mark the centennial of Yeshiva University. As a trusted member of the faculty and administration of the university, Gurock was given a free hand to develop his own research agenda and to approach the subject of the school's growth and development in a fresh way that departed from earlier histories of the school and its leadership. Gurock draws on these studies, but he uses Yeshiva University's centennial moment to explore the continuing dilemmas facing Orthodox Judaism in the United States. One finishes reading the book with a sense of the fragility of the compromise, *Torah U'Mada*, a "synthesis of secular and religious learning" (p. 5), that lies at the heart of Yeshiva University's educational philosophy.

Gurock situates the growth of American Orthodoxy within the changing historical framework of the development of competing institutions. The Yeshiva program, synthesizing traditional Talmudic study with elements of secular education, faced significant challenges for much of the first half-century of its existence from those who rejected all but the most minimal aspects of secular studies and from those who saw the training of rabbis and teachers as central. Although the former were identified with the Torah Vodaath Yeshiva in Brooklyn and the latter with the Jewish Theological Seminary in Manhattan, many of these critics were also Yeshiva supporters. The struggle for institutional self-definition involved simultaneously internal battles to define and implement an educational philosophy and external efforts to acquire legitimacy. Gurock relates this central drama, described with a balanced attention to the details of elite disputation, to social changes occurring among American Jews. His discussion of the stages of growth of Yeshiva's component schools, beginning with the predecessor to the Rabbi Isaac Elchanan Rabbinical Theological Seminary, illuminates the increasing complexity of Orthodoxy in the United States and the importance of the cultural baggage brought by different groups of Orthodox immigrants. Strong ties of men and ideas link the Old World with the New. Gurock's study reminds us that immigrant cultural history must pay more than passing attention to continuities.

The chapters dealing with developments after World War II focus on the changing social characteristics of American Orthodoxy as refracted through the students who entered Yeshiva. Gurock carefully describes the socioreligious situations of successive generations of students and indicates how the establishment of a complete parochial school educational network affected these young Jews. He also devotes a fascinating chapter to the 1960s that reveals how that turbulent decade touched a relatively insular Jewish community. A number of crucial links, however, are missing.

Gurock never quite explains which students were protesting or how the changed socioreligious background of American Orthodox Jews contributed to the crises of the late 1960s and early 1970s. One senses here the difficulties of doing contemporary history. Surprisingly, Israel and the 1967 war appear to have had little impact. But these minor shortcomings are outweighed by Gurock's major accomplishment of writing a centennial history that illuminates not only Yeshiva, its leaders and students, but also the development of Orthodox Judaism and higher education in the United States.

DEBORAH DASH MOORE
Vassar College

STEVEN M. LOWENSTEIN. *Frankfurt on the Hudson: The German-Jewish Community of Washington Heights, 1933-1983, Its Structure and Culture*. (A Publication of the Leo Baeck Institute.) Detroit: Wayne State University Press. 1989. Pp. 347. \$34.95.

Steven M. Lowenstein has long been troubled by conventional depictions of modern German Jews both in their homeland and in this country as, almost without significant exception, highly assimilated, far more enamored of German culture than of the ways and traditions of their people's past, wealthier than East European Jews, and always socially condescending toward their coreligionists. While still a student, he was seemingly disturbed that these almost universal depictions bore little resemblance to the type of transplanted German Jew he knew growing up in New York's Washington Heights section. And, as a historian, he has set for himself the scholarly agenda of underscoring how truly diverse German Jews were in their levels of economic and social mobility and in their degree of continued allegiance to ancestral ties and cultural norms. Ever the social historian, he made his case in his earlier studies by examining previously overlooked aspects of the origins of his group's encounter with modernity back in Germany. Now, in studying the refugee community of German Jews in northwest Manhattan, he attempts the very difficult task of "returning home," of examining an understudied community's history to determine with scholarly distance the actual levels of social and economic mobility and ethnic group persistence achieved and maintained by that community. Happily, Lowenstein steers clear of the snares of filiopietism and apologetics that have all too often undermined studies by American Jews of their own localities. Rather, he has benefited from knowing his neighborhood and has been able to combine the sensitivities of the "participant observer" with the skills of the trained social historian in producing a model of American Jewish communal history.

Lowenstein moves adroitly from delineating the varieties of both traditional and religiously liberal elements in the community to describing how residents greet each other, celebrate each other's birthdays, and conduct synagogue services. Significantly, he notes

that, although the Breuer neo-Orthodox community was, and remains still, the best-known Jewish religious group, it shared influence within the community with Reform, Conservative, and other Orthodox elements even as there were great commonalities in the social lifestyles of these several groups.

Equally important, Lowenstein is attuned to the impact of Americanization on the several generations of refugee and second-generation Jews. He not only looks thoughtfully at the degree of persistence of both Jewish and German cultural traits across several age groups but also takes great pains to compare the survival rate of such characteristics. His finding that Jewish identity has survived far better than the community's German personality is attributed to the network of Jewish socializing and educational agencies created neighborhood-wide. Interestingly enough, he finds that, although the desire to be German long remained a goal of the older generation, few formal efforts were made toward insuring such continuity of identity. That fact may be attributed to an implicit social contract made by these refugees with a largely accepting American environment into which they hoped to be integrated.

JEFFREY S. GUROCK
Yeshiva University

JOHN F. BAUMAN and THOMAS H. COODE. *In the Eye of the Great Depression: New Deal Reporters and the Agony of the American People*. DeKalb: Northern Illinois University Press. 1988. Pp. x, 230. Cloth \$25.00, paper \$9.50.

In the fall of 1933, Harry Hopkins, newly appointed director of the Federal Emergency Relief Administration, dispatched sixteen men and women, primarily journalists and creative writers, to observe firsthand the human dimension of the Great Depression. Chief among these investigators was veteran journalist Lorena Hickok, whom he charged to "go around the country and look this thing over. . . . I don't want the social-worker angle. I just want your own reactions, as an ordinary citizen" (p. 1). John F. Bauman and Thomas H. Coode present the results: over one hundred richly descriptive reports from every region of America, from blighted cities and ravaged countrysides, that detailed a nation's physical and spiritual suffering.

Bauman and Coode have produced a careful analysis of this impressive body of reportage, rightly arguing that it deserves to be included in the documentary tradition begun in the 1930s to comprehend the meaning of the depression for American culture. Taken together, the reports provided a vivid, anguished portrait of poverty, disease, despair, bureaucratic ineptitude, and capitalist callousness. Yet, loyal New Dealers all, Hopkins's reporters evinced a powerful faith that efficiently managed federal programs based on social engineering principles could solve many problems. Above all, they repeatedly advocated a national public works program to prevent the social degeneration of

millions of Americans whom free enterprise seemed unable to employ. Although Bauman and Coode have no direct evidence of the influence of these reports in the formulation of New Deal policy, they are probably right in maintaining that they bolstered Hopkins's case for the Works Progress Administration.

Hopkins commissioned these personal reports to gather "soft" information to supplement statistics supplied by his relief bureaucracy. Through personal observation and conversations with thousands of Americans, his reporters commented on how economic hardship was affecting the family and the potential for revolutionary movements among the unemployed. But, as Bauman and Coode admit, the reports often reveal more about the limited perspectives of their well-educated, middle-class, urban, and northeastern-based authors than they do about the feelings of the poor themselves. The writers interviewed local authorities more often than they did the unemployed. Obsessed by the need to distinguish between worthy and unworthy poor, they found it far easier to identify with the plight of unemployed white-collar workers and their families than with the chronically impoverished. The book is most affecting in its descriptions of such "stranded" Americans in rural backwaters, abandoned mill towns, and urban slums. Yet Hopkins's writers betrayed more than a little racism in their treatment of black Americans, and among poor whites they often blamed inbreeding and ignorance for the most extreme conditions.

Although engagingly written, the book at times threatens to deaden rather than quicken the reader's sensibilities through repetition, perhaps because of its organization into overlapping topical chapters. Nonetheless, in resurrecting a significant documentary resource, this study contributes to our growing knowledge of depression America.

SALLY GRIFFITH
Villanova University

DAVID T. BEITO. *Taxpayers in Revolt: Tax Resistance during the Great Depression*. Chapel Hill: University of North Carolina Press. 1989. Pp. xv, 216. \$29.95.

Taxes may be a "cost of civilization," but most of us would prefer our civilization at a discount. All the more so in the crash of 1929-33, when millions of desperately squeezed Americans faced ever more burdensome property tax levies. The consequent tax delinquencies and tax rebellion are the subject of this trailblazing study.

In a blockbuster introductory chapter, David T. Beito points to a broad base for tax resistance in the early 1930s, as revealed in a regrettably brief survey of the explosive growth of taxpayers' organizations, the notorious rural revolts against tax sales, and the campaigns at city and state levels to limit property taxes. Beito then focuses on two more limited topics: a taxpayer's "strike" in Chicago and the efforts to neu-

tralize anti-tax sentiment. In Chicago, most property taxes remained unpaid in 1931–32, and a thirty-thousand-member Association of Real Estate Taxpayers (ARET) piggybacked on this tax delinquency to spearhead an organized refusal to pay taxes. Beito ably records the local and national panic that this tax strike generated among such villains as city officials, bankers protecting their holdings in municipal bonds, teachers, and good-government “experts” who waged “pay-your-taxes” campaigns to discredit tax delinquents and build support for government. Tax rebels lost ground after 1933—a precipitous decline that Beito attributes to the absence of any systematic program for scaling back government along with such other factors as improved economic conditions, government concessions to previously aggrieved real estate owners, and tightened tax enforcement.

Beito would like readers to see this saga of rise and decline as evidence of a widespread animus toward big government among the depression-era public that was countered by an unsavory alliance of “tax spenders.” Despite impressive research, his evidence comes up short. As he notes, Chicago was a very atypical case, offering a foolproof recipe for tax revolts that scarcely required conducive public attitudes. Chicago in the early 1930s was in disarray, with wildly varying tax assessments and a tangled legal situation that eased tax dodging. This situation could demoralize even scrupulous taxpayers, who had better ways of spending their shrunken incomes. Beito does succeed in proving that tax resisters were not simply wealthy opportunists; his revealing examination of ARET’s membership shows a disproportionate share of small shopkeepers and skilled blue-collar workers. Yet his evidence suggests that ARET’s base largely derived from the class-action legal defense services that it provided. Beito’s tendency to identify ARET’s rank and file with the anti-big government statements of its leaders thus seems especially suspect.

Beito also gives readers too little reason to credit his conclusion that tax strike leaders damaged their cause by not going beyond general denunciations of bureaucratic inefficiency and calls for spending cuts to offer “specific proposals for privatizing government services” (p. 148). It is perhaps revealing that some of the author’s best evidence for anti-government sentiments by tax strikers comes from their opponents’ efforts to discredit them. One suspects that even many defaulting Chicagoans were more concerned than modern-day privatizers with the inadequacy of municipal services for the impoverished and unemployed (a context that emerges with conspicuous rarity in this book).

Such questions only suggest a future research agenda. The contributions of this useful study remain, not only in filling in the tax revolt story itself but also in reinforcing the emergent historical understanding of how the New Deal responded to, and was limited by, popular ambivalence toward big, unresponsive govern-

ment. No study of depression-era political culture should ignore this book.

MARK H. LEFF
University of Illinois,
Urbana-Champaign

HARRY A. KERSEY, JR.. *The Florida Seminoles and the New Deal, 1933–1942*. Boca Raton: Florida Atlantic University Press. 1989. Pp. xv, 214. \$24.00.

After years of relative neglect, the history of twentieth-century U.S. Indian policy has come into its own. The basic outline is now being filled in with tribal case studies. We know how the Indian New Deal affected the Navajo, the Iroquois, the Sioux (through oral histories), and now, with Harry A. Kersey, Jr.’s monograph, the Florida Seminole.

The Seminole hold a place in American tradition quite disproportionate to their numbers. They were Osceola’s people, the fighting Seminole who successfully resisted the government’s efforts to uproot them from their everglades homeland, and they remain a distinctive presence much touted by Florida tourist promoters. But, Kersey reminds us, for all of their fame, the Florida Seminole in the 1930s numbered fewer than six hundred individuals, about 0.2 percent of the Indian population nationally. Furthermore, the Seminole were divided into the Muskogee north of Lake Okeechobee and the Mikasuki to the south and spoke distinct languages. The Muskogee were generally more “progressive” in clothing, work patterns, and involvement with federal officials, the Mikasuki more traditional. The Indian New Deal in Florida faced the double challenge of reaching a small, widely dispersed population that was fundamentally divided.

John Collier, architect of the Indian Reorganization Act (IRA), bought into the Seminole romance on the two whirlwind inspection trips he made to Florida during his tenure as commissioner of Indian affairs, creating a paradox that Kersey neatly summarizes: “It was an interesting contradiction of the Indian New Deal that while official government policy espoused total Indian cultural and religious self-determination, it was simultaneously fostering programs on the reservations that would lead to their rapid social and economic assimilation” (p. 165). The peculiarity of the Seminole situation was how the tribal cleavage accommodated the New Deal’s contrary impulses. The Seminole were a special case for other reasons, too. They voted to accept the IRA (21–0) (p. 80) but did not act immediately to organize under its provisions, while reserving the right to do so by their acceptance. Their land base was stabilized, cattle provided, education promoted, and wage labor obtained for about one hundred adults under the Civilian Conservation Corps–Indian Division. The only issue with the explosive potential of the Navajo stock reduction program, albeit on a smaller scale, was the proposal to kill deer on Seminole lands to control a cattle tick, and here the bureaucracy worked

to stymie eradication. Thus, Seminole memories of the period tend to be favorable and bland, frustrating Kersey's efforts to incorporate the Indian viewpoint in an otherwise fairly traditional study of federal policy.

Tribal case studies are always welcome, and Kersey's book is carefully done. But its contribution to Indian New Deal historiography is limited by the fact that the Seminole population was, as he says, "miniscule" and the Seminole experience too contained and isolated to have broad application. Instead, the volume works best as "the second phase in a continuing study" (p. xiii) of Seminole history in the twentieth century. It ends with the Seminole emerging from the depression era intact, capable of bridging tradition and change, and prepared for the looming challenge of termination in the 1950s.

BRIAN W. DIPP
University of Victoria,
British Columbia

LAWRENCE C. SOLEY. *Radio Warfare: OSS and CIA Subversive Propaganda*. New York: Praeger. 1989. Pp. x, 249. \$24.95.

In a well-researched but overly long book, Lawrence C. Soley examines radio propaganda by both the Axis and the Allies in World War II. The broadcasts, whose purposes varied from lowering enemy morale to encouraging fifth column activities, came in three forms: "white propaganda," which was clearly identified as originating from a particular country; "gray propaganda," which had no identifiable source; and "black propaganda," which falsely identified its source. An example of the black propaganda was an Office of Strategic Services (OSS) station code-named "Charlie," which beamed broadcasts in 1945 from Kunming, China, to Japanese-occupied Canton. Operated supposedly by an underground, anti-Japanese resistance organization in the mountains near Canton, the station told listeners that its daily broadcast was designed to give them what they did not hear "over the lying Jap radio" (p. 180).

As Soley clearly shows, the broadcasts were relatively easy to put together compared to the struggle to get the permission to do them. The OSS frequently faced formidable opposition to the broadcasts from top military leaders such as Dwight Eisenhower and Douglas MacArthur, other U.S. government agencies, and foreign governments, particularly China. Each had their own interests and goals, and OSS broadcast requests were viewed as a nuisance at best and morally wrong at worst.

Although the book is interesting, it has serious flaws. Among the most striking are a wandering introductory chapter, which poses no research questions, and the lack of any conclusions at the end. For example, the main question is whether the OSS radio broadcasts, taken as a whole, played a significant part in the Allied victory in World War II. One wishes that Soley would

stand back and discuss this issue, but he never does. Thus, the reader is left with a vague view of OSS successes and failures.

In addition, Soley is confusing about his focus. The subtitle is misleading. The narrative is almost totally about the OSS, as the preface promises: "The book's emphasis is U.S. subversive warfare during World War II" (p. vii). The Central Intelligence Agency's radio propaganda broadcasts are virtually ignored. Also bothersome, and related to the lack of focus, is Soley's apparent inability to resist using everything that he collected in his research. At least a third of the information is superfluous. But the reader is forced to plod through it, all of the time wondering why it is included. A good editor with a sharp pencil would have helped immensely.

Other smaller flaws are the amazing number of agency initials, which continually force the reader to retreat to a list at the front of the book to decipher them, and the irritating use of two non-words, "psych-war" and "psychwarriors." These terms are more appropriate to science fiction than to scholarly history and should have been avoided.

Despite the flaws, military historians, as well as communications scholars, should find Soley's research valuable in evaluating government propaganda efforts in World War II. The book also suggests that there is still considerable research left to do in this area.

PATRICK S. WASHBURN
Ohio University

FRANK M. BUSCHER. *The U.S. War Crimes Trial Program in Germany, 1946-1955*. (Contributions in Military Studies, number 86.) New York: Greenwood. 1989. Pp. 197. \$39.95.

The aspect of Allied war crimes policy in World War II that, understandably, has been treated most generously and enthusiastically in the press and popular literature is the degree to which anti-Semitism and anticommunism inclined the Western Allies to go slow in catching and prosecuting Nazi exterminators. One unfortunate side effect of this phenomenon seems to be that little interest or energy has been left over for popular and scholarly examination of other features of the history of Allied actions against Nazi war criminals. Every inquiry into an additional phase, or new aspect, of war crimes history is therefore most welcome, and such a welcome is surely appropriate for the book under review.

Frank M. Buscher's title, however, exceeds by far the scope and scale of this volume. The development and form of the American war crimes policy is covered in a mere twenty-six pages. This introductory section is thin and contains too many errors of fact and omission either to provide a general introduction to the subject or to serve as an effective foundation for the rest of the book. These deficiencies are compounded by the lack of anything resembling an accounting of the policy's

immediate effects in regard to convictions, acquittals, and the prevalence of possible miscarriages of justice.

The book's real subject is the broad range of attacks that were made on the American war crimes policy between 1946 and 1955 and the responses to those attacks on the part of the American occupation authorities. In this area Buscher clearly breaks much new ground. His treatment of what might be called the "idealistic" American critics of the program, primarily judicial purists and civil libertarians, is brief but fair and insightful. The much longer sections on the post-trial clemency and appeal provisions established by the American authorities in Germany, as well as the attacks on the whole war crimes program made by private German groups and politicians of the Federal Republic, are far more comprehensive and thorough.

Although suffering from a confusing structure and a need for sharp editing, the book contains much useful material. It is an important scholarly building block, and, when joined by P. D. Jones's forthcoming dissertation from Cambridge University on the history of the British war crimes program, it will serve as the foundation for the next phase in the development of the subject, that is, a full-scale examination of the evolution of Allied war crimes policies, their implementation, and the controversies they engendered in all three Western zones of Germany during the first postwar decade.

BRADLEY F. SMITH
Cabrillo College

MICHAEL SCHALLER. *Douglas MacArthur: The Far Eastern General*. New York: Oxford University Press. 1989. Pp. xi, 320. \$22.50.

Despite the completion in 1985 of D. Clayton James's massive biography of Douglas MacArthur, the need remained for a solid one-volume study of the controversial general. For too long, William Manchester's engaging yet seriously flawed *American Caesar* dominated the popular market. Michael Schaller has taken up the challenge, focusing on "MacArthur's post-1935 activities in Asia as a means for understanding both the man and American foreign policy" (p. ix).

Schaller disputes Manchester's "worshipful" approach to MacArthur. Dispatching with the first fifty-five years of the general's life—his childhood, his career as a student and later commandant at West Point, his service in Europe in World War I, and his stint as army chief of staff—in a mere twenty-eight pages, Schaller draws extensively on James, the psychological study by Carol Petillo, and some juicy letters by MacArthur to his Filipino mistress. Schaller paints his subject as a well-connected man of limited talent and achievement combined with both a sense of destiny and a shaky self-esteem.

Schaller devotes more space to the next decade of MacArthur's life, although the wartime experience still merits only sixty-five pages. Schaller has no taste for the details of military planning and operations. He

consistently deprecates MacArthur's performance as a military commander and plays down the significance of his campaigns to the overall war effort in the Pacific. What interests Schaller most is politics, whether it be MacArthur's flirtation with the presidential race in the United States in 1944 and his cat-and-mouse game with Franklin Roosevelt or his maneuvering with Filipino elites on the eve of Pearl Harbor and after his return in late 1944. Schaller's linkage of MacArthur's military efforts with American politics makes fascinating reading.

Over half of the book is devoted to MacArthur and U.S. policy toward Japan, China, and Korea from V-J day to the election of 1952. Here Schaller builds on his important earlier study of U.S. policy toward Japan. Again his strength is in the area of American politics and foreign policy rather than military affairs. Schaller is adept at analyzing the connections between occupation policy in Japan and the presidential campaign of 1948, MacArthur's growing restiveness in Tokyo and his initiatives regarding Taiwan on the eve of the Korean War, and the Democratic strategy for undermining the deposed general when he returned to the United States in April 1951. In the last case, Schaller's use of materials at the Lyndon Johnson presidential library is particularly enlightening. Yet Schaller devotes merely a page to the planning, execution, and impact of the Inchon landing in Korea and totally ignores the personal relationships within the United Nations and Far Eastern Command in examining the debacle in North Korea during October and November 1950.

For the most part, Schaller is persuasive in his conclusion that "America's 'greatest expert on oriental psychology' . . . knew little about Asian realities and not much more about American politics" (p. 253), although it is hard to imagine many scholars needing much convincing on either point. Schaller also provides numerous tidbits of new information and insight. What he fails to do is to construct an integrated and reasonably comprehensive portrait of MacArthur. In addition to the limitations already cited, Schaller devotes little attention to MacArthur's ability to generate fierce loyalty among some of his subordinates and his almost mystical hold over the Japanese people after World War II. Schaller's preoccupation with American foreign policy sometimes comes at the expense of his treatment of MacArthur. Thus, although historians will read this book with profit, they are likely to emerge from it with the feeling that a good deal more could have been done with the topic.

Unfortunately, the publisher lacked sufficient interest in this project to ensure its production without an annoying number of printing errors.

WILLIAM STUECK
University of Georgia

JAMES A. HUSTON. *Outposts and Allies: U.S. Army Logistics in the Cold War, 1945-1953*. Selinsgrove, Pa.: Susque-

hanna University Press or Associated University Presses, Cranbury, N.J. 1988. Pp. 349. \$39.50.

James A. Huston is well known for his contributions to the study of recent American military history, which have focused on World War II. This book is probably his most ambitious project to date; Huston covers a significant topic in our understanding of the complexities that became the cold war. The book is surprisingly brief, considering the intricacies of Huston's subject. Although his emphasis is on the army's role in supply, of necessity he handles the broad outlines of the American logistics program, because much of his source material and the acts of Congress and other government agencies make it all but impossible to consider army activities apart from logistics.

The main contribution of this volume is that it will enable historians, particularly those concerned with cold war military developments, to understand how matters of logistics affected military strategy and thereby had a major impact on diplomacy. A second significance of his work is that Huston takes us through a labyrinth of facts and figures to support his general analysis of what was a truly agonizing process of American decision making, as officials of the Truman and Eisenhower administrations struggled to make sense of how the nation could establish a credible deterrence to what a majority of leaders, political and military, saw as an imminent Soviet threat. The author worked extensively in materials emanating from the Department of Defense and Congress and used a broad spectrum of interviews with military commanders who had major roles in policy formulation. One oddity—indeed a serious weakness of this book—is that Huston's research must have been undertaken primarily in the early 1950s. Very few of his sources, except for an occasional secondary work, come from any later point, and he has not used materials from presidential libraries or the modern military records branch at Suitland, Maryland. Using such materials would have rounded out Huston's research.

Focusing on the evolution of post-World War II defense strategy in the European theater, Huston provides a thorough picture of the difficulties Allied planners encountered in developing logistical support to meet the eventuality of a Soviet attack. Especially interesting is Huston's account of how the sale of surplus military equipment was integrated into planning for rebuilding a Western military presence in Europe. Huston points out that a much better job could have been done in salvaging more equipment globally if the logistical experts had not so frequently been mustered out of service. Huston provides a good account of the many roadblocks American negotiators faced in overcoming the objections of various European governments (particularly the French and Italian) to the creation of needed communications lines and base facilities for the armament program.

Huston includes substantial comment on problems that the United States faced in establishing an Asian

front against Soviet ambitions; his coverage of the China debacle and the ensuing war in Korea is especially strong. He also examines logistical matters in the Mediterranean and Latin America, although his coverage here is less comprehensive than his treatment of Europe and Asia. Throughout his book, Huston writes clearly, an asset for a topic of this complexity. My chief change in this study would be a summary chapter at the end. This topic would be much better fixed in the reader's mind had Huston done this. All in all, this study is a worthy contribution to post-World War II military history.

THOMAS M. CAMPBELL
Florida State University

PHILIP L. FRADKIN. *Fallout: An American Nuclear Tragedy*. Tucson: University of Arizona Press. 1989. Pp. xiii, 300. \$24.95.

During the past decade it has become increasingly clear that environmental issues will become major foci for public policy debates in the near future. The range of such problems is broad but has come to include air and water pollution, the disposal of nuclear wastes, and the as yet poorly understood effects of atomic radiation. Journalists, lawyers, and scholars in various disciplines have been quick to reflect this increasing concern. And, within just a few years, books by Howard Ball, A. Constandina Titus, Robert A. Divine, and Ernest J. Sternglass, among others, have examined the problems of nuclear fallout in the 1950s, although the impact on people, animals, and the environment was not recognized until thirty years later. This latest volume on the subject is by Pulitzer Prize-winning journalist and environmentalist Philip L. Fradkin, who has taken advantage of the Freedom of Information Act. His primary focus is on what he perceives as the government's deception of average citizens in Nevada, Utah, and Arizona who were unfortunate enough to be living in areas where the U.S. Atomic Energy Commission conducted secret atomic tests in the 1950s and who became unwitting victims of radiation.

As the author notes, his book had a circuitous publishing history. Fradkin began the project as a two-part series of articles for the *New Yorker*, designed to be published as a separate volume by Alfred A. Knopf. But neither the *New Yorker* nor the publisher accepted what Fradkin had produced. After various efforts Fradkin contacted the University of Arizona Press, which agreed to publish the work.

Fradkin's primary aim is to focus on what he charges was the federal government's betrayal of its citizens. He is less concerned with radioactive fallout and its cancer-causing effects. The centerpiece in the story is the case of *Irene Allen v. United States*, which involved residents of states where the government had conducted atomic tests. Begun in 1979, the proceedings were not concluded until 1988. The testimony in that case brought about a greatly increased public awareness of alleged

injuries sustained by people who had lived in Nevada, Utah, and Arizona in the 1950s and who developed cancer in later years. Fradkin examines what he considers "the crime" in nine terse chapters. As a skilled journalist, Fradkin slowly builds up the drama of his story. He begins with his indictment, namely the dimensions of "the crime" and how federal officials sought to hide it from public view. He then examines the gradual discovery of the harmful effects of radiation and the varied efforts of Congress and the federal bureaucracy to hide the evidence and not even acknowledge the health hazards of early atomic tests. At least one-half of the book deals directly with the lengthy trial at which former residents in test areas testified about alleged injuries they had suffered because of nuclear fallout. They also requested federal compensation for their injuries.

The volume is well documented. Fradkin sat through many hours of the trial and also conducted extensive interviews with many of those involved, including witnesses, victims, lawyers, and judges. He combed mountains of written documents, published and unpublished, whether in trial records, archives, or agency and manuscript collections. The result of these labors is an opinionated but well-documented account of one of the federal government's best kept secrets. Despite court decisions upholding the sovereign immunity of the federal government, it is unlikely that the issues involved in nuclear testing have simply been laid to rest. This volume is a useful addition to the growing body of literature on what is clearly a highly charged and emotionally laden subject.

GERALD D. NASH
University of New Mexico

LEON FINK and BRIAN GREENBERG. *Upheaval in the Quiet Zone: A History of Hospital Workers' Union, Local 1199*. (The Working Class in American History.) Urbana: University of Illinois Press. 1989. Pp. xvii, 298. Cloth \$24.95, paper \$9.95.

American labor historians have generally neglected the period since 1945. This pathbreaking history of Local 1199, which began organizing hospital workers in 1958, illustrates how advocates of the new labor history can convincingly reconstruct recent worker struggles from the bottom up. Written by two historians whose previous publications concentrated on nineteenth-century workers, this study focuses on the people—rank and file members as well as leaders—who turned a union for professional pharmacists into an agent of change for unskilled hospital workers. Leon Fink and Brian Greenberg clearly portray the social and political climate that made change possible.

Hospital workers, who were effectively excluded from New Deal labor laws, had long worked for less than the minimum wage and under nineteenth-century conditions. Primarily black and Hispanic women, they took low-paying service and maintenance jobs as a last

resort, and they compiled annual turnover rates as high as 80 percent in New York City's private, non-profit hospitals during the 1950s.

These unlikely union recruits found a collective voice in Local 1199, a small union of drugstore employees in New York City. Led by Jewish radicals who had survived cold war purges, 1199 took advantage of a number of developments to organize hospital workers first in New York and then in other cities from Philadelphia to Charleston. Fink and Greenberg stress that, in addition to rank-and-file militancy and brilliant leadership, 1199's success also depended on external forces, especially the civil rights movement, which radicalized black workers, and increased government spending for health care services, which subsidized hard-fought union contracts during the 1960s. While emphasizing economic issues, Local 1199 also provided a strong sense of racial and ethnic pride through educational and cultural programs for minority members. The union, growing to a national membership of one hundred thousand in 1980, was committed to "the effective joining of 'union power' and 'soul power'" (p. 129).

This sympathetic history, based on union records and revealing oral histories, does not hesitate to point out the weaknesses of Local 1199, some of which explain the internecine warfare that fractured the union in the 1980s. Internally, the union had a highly centralized bureaucracy with extraordinary powers residing in the presidency. When Leon Davis relinquished that office in 1981, he designated Doris Turner, a black woman, to head the national union's power base in the New York district. Personal and policy differences soon produced a formal split between the New York district and the national union at a time when hostile economic and political conditions forced containment of rising health care costs. The authors also note that "traditional male dominance within the union power structure undoubtedly added to the problem of forming a new regime" (p. 211), but throughout the book they fail to explore gender issues in a union that was three-quarters female.

Despite this noticeable shortcoming, Fink and Greenberg's book is a highly readable study that shows how one group of working poor gained some control over their lives. The book will be of considerable interest not only to specialists in labor, black, and contemporary history but also to their students.

ROBERT P. INGALLS
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KENNETH O'REILLY. *"Racial Matters": The FBI's Secret File on Black America, 1960–1972*. New York: Free Press of Macmillan. 1989. Pp. viii, 456. \$24.95.

Since within this decade David J. Garrow and Richard Gid Powers have provided two excellent studies of J. Edgar Hoover and the Federal Bureau of Investigation (FBI), a would-be reader of Kenneth O'Reilly's

study might wonder if yet another examination of Hoover's FBI activities is justified or necessary. The answer is a resounding yes. Garrow's *FBI and Martin Luther King, Jr.* is a comprehensive but limited study of the 1960s civil rights leader's confrontations with Hoover, and, although Powers's *Secrecy and Power* also includes discussion of the FBI's involvement in civil rights, it is essentially biographical. O'Reilly attempts to produce an in-depth account of the FBI's role—from silent observer to active agitator—in the civil rights movement, and he creates a superbly written study.

Although Powers deals with Hoover's well-known racism at length, O'Reilly demonstrates how unrealistic it was to expect Hoover—who was convinced that the 1954 *Brown* decision would put "the country on a course toward racial crisis"—to direct his agents to protect civil rights workers (p. 41). On the contrary, the FBI's numerous transgressions during this critical period are mind-numbing, and the author makes it clear that, because Hoover was incapable of putting aside his own prejudices, the nation's most esteemed law enforcement agency actually exacerbated the situation.

Hoover's resistance to the civil rights movement, it seems, took two forms. On the one hand, it was passive, claiming, for example, that a federal agency should not interfere with state laws and prohibiting agents from making arrests, thus limiting them to investigative work. On the other hand, Hoover's interference was more direct, as on the occasions when the FBI would not cooperate with the Justice Department's civil rights division. Hoover wanted to derail not only the civil rights movement but its leaders as well. To accomplish this, Hoover eventually approved the use of counter-intelligence tactics in Division Five, within the domestic intelligence division, as well as "all possible investigative techniques" to expose the "communist influence on the Negro" (p. 136). The primary target of the operation, of course, was Martin Luther King, Jr., whose home in Atlanta and the Southern Christian Leadership Conference offices in New York were under the surveillance of fourteen wiretaps, none of them approved by Attorney General Robert F. Kennedy. In addition to the wiretaps, Division Five also proposed to infiltrate the King camp with black agents, although O'Reilly does not elaborate on whether this was actually done.

Many of the episodes, though tragic, are familiar: the murder of Emmett Till for whistling at a married white woman in Money, Mississippi; the brutal assaults against freedom riders in Anniston, Alabama; the bombing of Birmingham's Sixteenth Street Baptist Church that killed four young girls; the murders of three civil rights workers in Neshoba County, Mississippi; and the shotgun assassination of Viola Liuzzo, who Hoover once claimed was promiscuous and "had indications of needle marks in her arms" (pp. 218–19). Although both allegations against Liuzzo were proved false, this kind of defamation was indicative of Hoover's unsympathetic attitude toward white sympathizers as well as black "agitators." Although these events and their outcomes are commonly known,

O'Reilly never fails to capture and sustain the reader's interest with a well-paced and smoothly written narrative of the grim reminders of the price of freedom.

In terms of Hoover's posture on civil rights, the author cites the 1963 March on Washington as a watershed that convinced Hoover that the civil rights movement was not ephemeral, that it was more than a haphazardly organized group of communist-inspired radicals, and that it would have to be destroyed before it caused irreversible damage to Hoover's America. When Hoover realized that communists were not deeply involved in the movement, the FBI did not back off but pressed even harder to find evidence of their involvement.

Like Powers, O'Reilly is very much aware of Hoover's lifelong concern for "his" America and the vision of himself as the preserver of traditional American values. The civil rights protestors threatened these values because they "were not part of the real America, 'the hard-working, tax-paying, law-abiding people of this country'" (p. 258). That O'Reilly frequently reminds the reader of this is a very minor flaw in what is otherwise a provocative and well-conceived contribution to our understanding of the struggle for civil rights.

Perhaps the most disturbing question that comes to mind while reading O'Reilly's account is not how Hoover worked to discredit black leaders by linking them to communism, or how he labored to avoid direct intervention, but how much violence and bloodshed might have been avoided had the director ordered his agents to protect the activists rather than spy on them as suspected subversives. Because Hoover maintained that the FBI was an investigative and not an enforcement agency, he impeded the movement and contributed to political and public misconceptions about civil rights.

This study (the title is taken from the file Hoover used to gather information about suspected black troublemakers) is extensively researched as evidenced by O'Reilly's use of the Freedom of Information Act to procure documents and his interviews with many FBI agents and participants in the civil rights movement. O'Reilly succeeds in producing a vivid and insightful look at how this nation's premier law enforcement agency stood as a barrier against black Americans' struggle for equality and participation in the democratic process.

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CANADA

J. R. MILLER. *Skyscrapers Hide the Heavens: A History of Indian-White Relations in Canada*. Buffalo: University of Toronto Press. 1989. Pp. xi, 329. \$35.00.

Canadian historical writing in regard to Amerindians had its moment of truth with the publication in 1971 of

the first of James Walker's articles pointing out the deplorable treatment of New World peoples. Walker's papers accelerated a trend that emerged after the Second World War to reassess accepted historical assumptions. It is from selected items in this body of material that J. H. Miller's book is drawn.

With earnest dedication to the Amerindians' cause, Miller has synthesized these secondary works to survey Amerindian-white relations in Canada from the days of Jacques Cartier (following a brief mention of the Norse) to the present. As he reports it (p. 40), harmony and cooperation prevailed at the beginning as the French were only interested in "fish, fur, exploration, and evangelization" (apparently he has not read the royal commissions of Cartier and Jean-François de La Roëque, Sieur de Roberval, among others), a happy situation that disintegrated through time and various political tides until the Amerindians became a marginalized people, a condition from which they are only now emerging.

Instead of judiciously examining the basic issues involved in the first contacts between Europeans and Amerindians, Miller has allowed his desire to do justice to the Amerindians to overtake his sense of history, and we are treated to a version of first contacts as they might have been. His wonder at the cooperation that existed (p. 20) should be tempered by the fact that cooperation was by no means universal, as even a cursory look at the record quickly reveals.

He also falls in line with some recent attempts at "rectifying" the record that would be better forgotten. One of these is to try to remove the sting from the appellation "sauvage" by restricting its meaning to a neutral ground. If he had consulted sixteenth- and seventeenth-century sources, he would have found that "sauvage" had a complex of meanings; one of the kinder was "living according to nature," which was equated with existence without law and order, "like beasts in the woods." Once a "sauvage" was Christianized, he was "humanized" and thus no longer fully a savage. Miller's statement that Amerindians were "perceived positively on the whole" (p. 94) is simply not supported by the evidence.

An obvious consequence of these attitudes was the assumption on the part of European monarchs that they had the right to claim vast tracts of New World lands regardless of whether or not there were Amerindians already living there. When Miller states that the French "did not assert title to land and political sovereignty over the indigenous people with whom they came in contact" (p. 257), he is ignoring not only these royal charters but also the fact that French settlements were of necessity in Amerindian territories. He does, however, admit that this expansion was done by means of legal fictions to which Amerindians were not privy.

The British, when they took over from the French, lost no time in issuing the Proclamation of 1763, which assumed underlying British sovereignty over Amerindian territory, a direct consequence of those early notions about the nature of Amerindian occupancy.

Canada's first aboriginal rights case, *St. Catherine's Milling v. The Queen*, fought between 1885 and 1889, clearly restated these ideas. The court went along with them and ruled that Amerindians had no proprietary title to the soil. A lone dissenting opinion held that Amerindians had "a personal and usufructuary right, dependent upon the goodwill of the Sovereign." Far from breaking new ground, as Miller implies, this was a reaffirmation of a principle that had already been acknowledged in earlier treaties, such as those of Portsmouth in 1713 and Boston in 1725. It is an early version of aboriginal right, a concept that in Canada remains to be fully defined either by the Constitution or the courts.

Historian John Huxtable Elliott once observed that much historical writing today is bogged down in philosophical speculation and historical assertion rather than rigorous historical enquiry. It is to be regretted that Miller has followed this trend, because his purpose is praiseworthy. The Amerindian side of Canada's history does need to be told but with the same high standards for investigative rigor that is demanded of the discipline in general.

OLIVE PATRICIA DICKASON
University of Alberta

PETER DOUGLAS ELIAS. *The Dakota of the Canadian Northwest: Lessons for Survival*. (Manitoba Studies in Native History, number 5.) Winnipeg: University of Manitoba Press. 1988. Pp. xvii, 262. \$34.95.

The historical literature of American Indians contains numerous volumes on warfare and policies. Some studies focus on cultural change, but rarely do they deal with the many ways in which Indian people adapted to their rapidly changing world. Peter Douglas Elias has provided an excellent work that details how the Dakotas survived in the Canadian Northwest after facing the onslaught of expansion, war, and Indian policy in the United States. The Sioux in Minnesota lost the war in 1862, and, by year's end, some of them began to move into Canada. First, Little Crow's band migrated, and then others followed, seeking sanctuary on British soil. By 1865 the Dakota bands had become distributed in a pattern that remains largely unaltered to this day. The Dakotas settled on land once claimed by the Cree, Métis, Assiniboine, and Ojibway. Part of their existence depended on the successful peace established between themselves and the various tribes in the region.

The Dakota bands moved onto land that they considered most suitable for their survival. The M'dewakonton occupied territory close to the forests, while the Sissetonwon moved onto the prairies. The Wahpeton chose land in between that contained both forest and grassland. Those choices represented adaptations to environments similar to the ones the bands had known in Minnesota. Between 1862 and 1870 the Dakotas used their prowess to survive in Manitoba. The Indians

drew on their own knowledge and that of whites to adapt. Some of the Indians continued to farm, while others learned to till the earth. The M'dewakonton band and the Dakotas at Turtle Mountain primarily earned their living by hunting, fishing, and trapping. The Dakotas living near Fort Ellice also raised cattle and cut wild hay. Some of the Indians were wage earners, and most of them cultivated small gardens. In these ways the Dakota survived in Canada, in spite of the fact that the British crown refused to recognize Dakota claims and aboriginal rights.

Elias provides chapters examining each of the Dakota bands, detailing their economic and political history. For example, he offers specifics on how the Dakotas on the Oak Lake, Oak River, and Birdtail reserves became successful farmers, while the Turtle Mountain people maintained their economic tradition as hunters and gatherers. Elias also offers substantial evidence that the Dakota used the resources of the Canadian Northwest long before their recent movement into the region, arguing that Dakotas have lived in Canada for over eight hundred years. Elias clearly explains the intricacies of Canadian-Indian policies and their implications for the Dakota. The work is based on solid archival research in Canada, England, and the United States. In addition, it is enriched by oral histories of tribal elders. The author has used many sources to weave a masterful study that is unique and informative. It is a welcome addition to the literature of the North American Indian.

CLIFFORD E. TRAFZER
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D. N. SPRAGUE. *Canada and the Métis, 1869–1885*. Foreword by THOMAS R. BERGER. Waterloo, Ont.: Wilfrid Laurier University Press. 1988. Pp. xii, 204. \$17.50.

Studies of Louis Riel, the Métis people of western Canada, and the two "rebellions" of 1869–70 and 1885 are numerous. Indeed, analyses of Riel have formed a sort of psychological and symbolic battleground in Canadian history. Thus, practically from the time of his execution in 1885, works have poured forth defending him, condemning him, or seeking an understanding of the sources of armed resistance to what was generally a peaceful Canadian occupation of the western territories. The question then is what new perspective this book can bring to the existing material.

According to D. N. Sprague, what the book brings is a suspension of an assumption of "government benevolence" and Métis "primitivism" (pp. 16, 17). Authors of previous works have been blinded either by bias or by a failure to follow the logic of the evidence. Thus, Sprague sets himself the daunting task of overcoming what he argues is the tendency to support, though not completely condone, the governmental position in the affair.

In both a historiographic and a specific context, the book seems to have a definite purpose. Throughout the

work Sprague implies that government hostility toward the Métis led to a conspiracy by Canadian prime minister John A. Macdonald. In 1885, according to this conspiracy theory, Macdonald actively or passively encouraged rebellion in order to show the military necessity of the nearly bankrupt Canadian Pacific Railway and thereby gain support for a further subsidy. If true, then Riel and the Métis were sacrificed at the hands of a cynical prime minister.

Unfortunately, the book fails for specific reasons. First, Sprague's assumptions both about the historiography and about his own position in it are not completely accurate. Works have ranged over a wider ideological and historiographic position than he allows. Scholars of the issue of Riel and the Métis have formed two active camps with many gradations in between. The first camp is, it is true, characterized by support for the basic government position. Equally important, however, there has been a second stream of writing that has supported Riel and the Métis and condemned the government. Even the conspiracy theory about the Canadian Pacific is not original. Thus, Sprague has not carved out some new and neutral position; he falls into another well-developed tradition, namely, supporter of the Métis and Riel.

Such a stance is understandable given the lamentable living conditions after 1885 under which the Métis have struggled. There are also grounds on which the Métis can realistically be defended. Having taken a pro-Métis position, however, Sprague is burdened by his desire to prove the conspiracy theory. Here the problems in the work become more serious. First, the book has an unusual structure. Less than 20 percent of it deals with the 1880s, although that period is central to the thesis that Sprague is developing. Perhaps the most basic problem of all is that, unlike some popular historians, Sprague cannot bring himself to ignore the evidence. And the evidence simply does not support the conspiracy theory. Thus, Sprague is left with only the weakest techniques to bolster his case. He is forced to impute motivation without solid evidence, especially in the case of Macdonald. Second, also in the absence of solid evidence, he uses loaded words such as "duplicity" and "provocation" to try to set a tone when describing the government. Finally, Sprague's desire to support the Métis occasionally leads him to abuse historical evidence. In the most spectacular example of this problem, he tries to explain away the execution of Thomas Scott by a firing squad, saying that it was all an "accident" (p. 50).

Sprague can conclude only that his study indicates that a thesis of government benevolence is not supportable. This is true enough but hardly new. When he also admits that a supposition of benevolence cannot be replaced with an assumption of malevolence (p. 184), it becomes apparent that, for all of its detail, this book adds little to what is already well known about the period.

DOUG OWRAM
University of Alberta

DAVID MILLS. *The Idea of Loyalty in Upper Canada, 1784–1850*. Buffalo: McGill-Queen's University Press. 1988. Pp. x, 238. \$27.95.

David Mills has attempted the task of tracing the "idea of loyalty" in Upper Canada from 1784, the founding of the first loyalist settlement, to 1850, when the age of responsible government was underway. This volume forms a useful preliminary to Carl Bergers's *Sense of Power*, which deals with the ideas of the Imperial Federationist in the 1880s. Mills has done exhaustive research, helped in some degree by recent monographs, including the thirty-one graduate theses that he lists in his bibliography. These at least were useful in leading him to the relevant original sources. On the whole, it is a long story well told. Mills at times hovers on the frontiers of Whig history but keeps his independence and makes no effort to squeeze his material into a Marxian strait jacket.

In the early history of Upper Canada, a group of office holders, called by their enemies and most historians "the Family Compact," attempted to assert monopoly on loyalism and denounce all opposition as "republican" or "annexationist." This effort was never accepted at face value by the majority of the province or by the colonial office. The intellectual spokesman for this party was John Strachan, who became the first Anglican bishop of Toronto. The members of the Family Compact were united in a belief in a strong executive, which was inspired by the American *Federalist*, and a conviction that an Anglican-oriented system of education would be a civilizing influence necessary to keeping the province British.

Loyalty did not become a political issue until the election of 1836, when the lieutenant governor appealed to the electorate to return loyal members to the assembly. This brought Methodists, a Roman Catholic bishop, and an Orange grand master along with many erstwhile reformers and Compact Tories into a temporary coalition in support of the government. A year later blacks, Indians, and Irish immigrants turned out as loyalist volunteers against Mackenzie's "Rebels."

Although the Compact benefited by this coalition as its members remained in office, they were out in 1840 when another governor organized a governor's party that included liberals. In 1844, when still another governor put together a party, the Compact members had to accept a partnership with outsiders such as John A. Macdonald and abandon their claim to a monopoly on loyalism.

All of this is explained clearly by the author. This volume will be a useful text and essential reading for those who take a long look at Canadian history. The conclusions that Mills presents are cautious, and there is room for more speculation than he offers. Were those who saw British institutions as the best road toward the future more conservative than their republican counterparts? Some of the "loyalists" such as the Upper Canadian reformer George Brown considered the republic to be illiberal. So, too, did the Canadian

abolitionists. Moreover, the "idea of independence" rested on the facts of geography, "the idea of loyalty" on the facts of cultural origins. Both sets of facts are permanent. The big battalions have been on the side of independence, but there was more to "the idea of loyalty" than clinging to a colonial past.

HEREWARD SENIOR
McGill University

J. K. JOHNSON. *Becoming Prominent: Regional Leadership in Upper Canada, 1791–1841*. Buffalo: McGill-Queen's University Press. 1989. Pp. xii, 277. \$29.95.

J. K. Johnson has produced a collective biography of 283 Upper Canadians elected to the Upper Canadian House of Assembly at some point in the period from 1791 to 1841. He attempts to analyze the economic, social, and political characteristics of members of this elite to understand better the reasons for their prominence. The sometimes fragmentary nature of the data leads Johnson to emphasize that this study is not a sophisticated quantification, nor are the conclusions necessarily definitive. Nonetheless, it is an exhaustive and thoughtful dissection that clarifies, confirms, and at times revises prevalent assumptions about the Upper Canadian elite in particular and Upper Canadian society in general.

Prominence is not given a precise conceptual definition but merely described as a term indicating that the individuals enjoyed "greater recognition" and "played more active roles" than most of their contemporaries (p. 9). Studies of the nature of the Canadian elite covering a later time period, such as John Porter's classic *The Vertical Mosaic* (1965), emphasize the common ethnic and religious roots, the charter group status, so vital to understanding the makeup of the sociopolitical and economic rulers and decision makers. Johnson's work confirms that factors such as family linkages, Loyalist descent, and Anglicanism were keys to economic and political success in Upper Canada as well, and it is clear that certain national groups did better than others and were given better treatment.

The occupational pluralism necessary for success in early Upper Canada is established, but, although some combination of farming, public service, storekeeping, milling, and law activities among the Conservative elite was initially apparent, these occupations were gradually supplanted or reinforced by mercantile, legal, manufacturing, and entrepreneurial enterprises conducted in an urban rather than a rural environment. These changes were in keeping with the changing nature of Upper Canada. On the other hand, the careful control exerted over certain groups, or even their exclusion from power, remained largely unchanged. Despite the Americanization of Upper Canada in the period from 1792 to 1812 and the later influx of Irish immigrants, the Irish and the Americans were never entrusted with even a shadow of the power or influence that was representative of their numbers.

Indeed, the Loyalist influence that had declined before the War of 1812 reappeared in the period between 1816 and 1820 and later as sons of Loyalists made their appearance and impact. The garrison mentality of Upper Canadian leaders in the face of any American or anti-British threat became stronger rather than weaker. Descriptions of Canadians as an unmilitary people are far off the mark among this elite; 88 percent of Conservative members of the assembly between 1828 and 1841, and 72 percent of the Reformer members (p. 77), served in the military. To the voters, apparently, this characteristic was both a "virtue" and an "indispensable requirement" (p. 132). This colonial mentality was further demonstrated in the tendency to turn to immigrants from the United Kingdom, other than the Irish, of course, when political representatives were chosen. Immigrants from the United Kingdom always made up around 30 percent of the Upper Canadian House and even increased that representation toward the end of the period under examination.

The limited definition of prominence may cause readers to question whether any study that deals only with elected political representatives captures all of those shaping that society. Educational and religious figures, significant American entrepreneurs, and lieutenant governors and their closest appointed advisors are given short shrift with such an approach. Nevertheless, Johnson has authored a rewarding and suggestive book. There is a need for comparative studies of the prominent in other parts of British North America, for example, Atlantic Canada. The *Dictionary of Canadian Biography* (D.C.B.), it should be noted, has been assiduously employed by Johnson. His book, one hopes, will be one of many studies on pre-Confederation Canada made possible and enriched by the massive D.C.B. project, which, in the twelve volumes now published, has treated significant Canadian historical figures in the period between 1000 and 1900.

W. G. GODFREY
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J. RODNEY MILLARD. *The Master Spirit of the Age: Canadian Engineers and the Politics of Professionalism, 1887-1922*. Buffalo: University of Toronto Press. 1988. Pp. 229. \$30.00.

Modern engineering is quintessentially an industrial occupation. Focusing on the persistent lack of concern among engineers for "social responsibility," Edward Layton's *The Revolt of the Engineers* (1971) raised disturbing historical questions about whether engineers had a collective professional identity or were members of a self-serving trade. Indeed, the "revolt" was, he suggested, a generational rebellion in the name of a noble professionalism by which the new middle class of organization types desired to take status, money, and power away from the older entrepreneurs. Many of the themes raised by Layton have subsequently been developed by Bruce Sinclair, Monte Calvert, Thomas

Hughes, David Noble, A. Michal McMahon, and others. For instance, in case after case in a workplace where information about the harmful effects of technology was suppressed at the cost of human tragedy, salaried engineers willingly placed employer demands above professional skill and judgment. (The Challenger disaster, public deception by a big oil company concerning its ability to manage spills, or false testimony by the nuclear power industry about safety will surprise no one familiar with the history of subservience in America's technicians.) In part, the historical problem has been that there were too many diverse fields of engineering drawing on widely different levels of science. In various types of industries, many with strong trade associations, individuals scrambled in corporate hierarchies in order to rise above engineering into management. Engineers as a result were both difficult to organize professionally and indifferent to a professional identity beyond the material benefits of a successful public image.

J. Rodney Millard contributes to this study by focusing sharply on the institutional politics of a small but influential group of civil engineers in Canada between 1887 and 1922. This is a book about engineers, not engineering. It is about, in Millard's terms, a middle class of "unabashed apologists for an acquisitive, materialistic society" using the "outward pretense" of professionalism as "merely a pragmatic political ploy to acquire professional regulatory power" in order to serve a special interest (pp. 11, 146). Despite some obvious cultural differences, including a more rigid social structure and variations in the timing of developments, the contours of the Canadian pattern resemble the American one.

At the outset, an older generation of self-made entrepreneurs with a laissez-faire business philosophy was in control. Anyone could practice. The average engineer living on a fixed income was stigmatized in the Victorian mind as a tradesman or manual worker performing industrial labor. This frustrated an aspiring middle class, which felt itself to be unrecognized and unrewarded. Bolstered by a belief in itself as the natural leader or "master spirit of the age" solving the problems of the new industrial order with engineering expertise, this class determined that it could improve its position by pursuing the familiar process of professional developments: technical knowledge based in schools and journals, an ideology of performing essential public service, and an engineering vision of material abundance achieved by means of technocratic efficiency. But this was secondary. The real fight for power, according to Millard, occurred at the organizational level. By preferring the monopolistic powers of professional licensing bodies, the new generation of upwardly mobile engineers succeeded in both curbing the self-made business person on the one hand and rejecting unionization or any association with the working class on the other. Professionalization, Millard generalizes, was a uniquely middle-class solution to frustrations over status within industrial capitalism.

It should be noted that the narrative is narrowly conceived, often reading like a dissertation. Two criticisms arise. First, Millard's focus is so exclusively Canadian that slight reference is made to the historiography except for Layton's book. An opportunity was missed to discuss similar patterns and problems in a larger context. Second, and more serious, Millard's use of the category "middle class," which is so essential to his argument, never emerges beyond a fiction. It is no more complicated than the caricature of an upwardly mobile consumer and comes across as no more actual than a collective statistical biography or an ideological pronouncement.

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PETER NEARY. *Newfoundland in the North Atlantic World, 1929–1949*. Kingston, Ont.: McGill-Queen's University Press. 1988. Pp. xi, 459. \$32.95.

Newfoundland became Canada's tenth province in 1949 after fifteen years of a unique departure from parliamentary government; it was the only province to join the Confederation because of a collapse of its own representative institutions. Newfoundland's vulnerable economy, based largely on fish and wood products, forced its government so deeply into debt that in the early 1930s it faced bankruptcy, an economic fate that both Newfoundland and its former imperial mentor, the United Kingdom, were anxious to avoid at almost any political cost. The final result, after extensive official inquiry, was the abandonment of responsible self-government and its replacement by an appointed commission of six members (three each from the United Kingdom and Newfoundland) presided over by an appointed governor.

Peter Neary, with admirable clarity, has chronicled the history of the commission from its birth pangs to the disposition of its members after Newfoundland became a province. It is not an easy tale to tell, because it involves a tangled web of economic, political, and constitutional problems that together make plain how complex public affairs can become in a jurisdiction whose population, though no larger than that of a medium-sized city, was scattered along a coastline and deeply divided on sectarian and social grounds. Technically, in 1931, Newfoundland had become a self-governing dominion, with "full power to make laws having extra-territorial operation," by virtue of the Statute of Westminster, the legal basis of the Commonwealth of Nations. Reducing that arrangement to government by a nonelected committee of six, half of them from another member of the Commonwealth, presented its own difficulties, but none compared with the day-to-day realities with which the committee had to cope.

The commission took over an island with a massive debt in the middle of a world-wide depression and

suddenly, with the outbreak of war in 1939, found itself grappling with almost the opposite, a new-found prosperity that included fresh negotiations with other countries, because the technology of the time made Newfoundland an indispensable strategic location for air and sea travel. The United Kingdom was joined by Canada, and then the United States, in seeking influence in Newfoundland, and the location of bases, especially, raised questions about who really exercised sovereignty over the island. The carry-over of wartime prosperity into postwar reconstruction inevitably led to speculations about the future, and in due course, after a "national" convention had deliberated, the electors were asked to choose among a return to responsible government, the Confederation, and a continuation of the commission. Responsible government won but without a clear majority (44.55 percent), and, in a run-off referendum, after vigorous partisan campaigns, the Confederation won narrowly (52.34 percent).

The commission was clearly unpopular (only 14.32 percent in the one ballot it was on), largely for reasons beyond its control. Apart from the problems leading to its creation, the commission had to govern a volatile political community without the aid of a representative—let alone a party—system. (If the book can be faulted, it is because the author says little about how the commission handled such time-honored political matters as patronage.) From its birth the commission was doomed to give priority to ad hoc solutions for immediate problems.

Nonetheless, among other achievements, it did attempt to reform the civil service, develop a land policy, help Newfoundland with meager but better social measures in the depression, and create a surplus for postwar reconstruction. The commission maintained an identity for the island's government, which could have been lost. Confederation alone, preceded by lengthy consultations, was "a complex diplomatic, constitutional, and political event," which gave Newfoundland a "greater independence . . . than it could have achieved on its own" (pp. 344–45).

Neary does his tale full justice, skillfully threading his way through a labyrinth of detail in well-turned prose that reveals his concern for his subject. His book is a rewarding study of a singular aberration in the history of representative government.

NORMAN WARD†
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LATIN AMERICA

CARLOS MARICHAL. *A Century of Debt Crises in Latin America: From Independence to the Great Depression, 1820–1930*. Princeton: Princeton University Press. 1989. Pp. xiv, 283. Cloth \$45.00, paper \$16.50.

Carlos Marichal's main thesis is that Latin American debt crises occur because of trends in developed nations rather than because of policy errors by borrowing

nations. Marichal presents evidence on foreign borrowing for several nations but does not use economic histories of the Latin American nations included in his book, nor does he present crucial evidence in the form of tables indicating Latin American government deficits or gross product and export price levels for Latin American or major capital exporting nations. Instead, Marichal retells reports of excessively high commissions and stock market manipulation by those selling Latin American government obligations in the early nineteenth century. He places less emphasis on the inadequate supervision of how the borrowed funds were spent. Instead, Marichal complains that "the loans injected capital into Latin America. . . all the debtor nations were committed to pay back a sum much larger than what they had originally received. Such circumstances made the debtors easy prey for an ever-growing circle of foreign financiers who urged them to take more loans with which to pay off previous debts. . . an explosion became inevitable" (p. 6).

Successful developing nations—such as the United States until World War I—were in debt. They used the money to finance development, although problems arose if loans had to be repayed when export prices received by borrowers were low. In these days the International Monetary Fund makes loans to counteract unforeseen shortfalls in receipts of leading exports. Nineteenth-century commercial banks did not. If commercial banks are not always appropriate sources of development funds, how can they be obtained? Colonies obtained funds for infrastructure from imperial governments. In the nineteenth century, some Argentines complained that they were not part of Britain's official empire. Funds also are obtained by equity investments. In some cases, migration was an important factor transferring resources and creating demand for funds.

Both provincial and national governments borrowed money. Yet, in some cases, their inability to repay was linked neither to their own policy errors nor to European financial conditions. For example, the outbreak of sheep disease in Argentina in the late 1880s reduced export earnings. Argentina consequently defaulted on payments to Baring, which in turn triggered an international financial crisis. That type of occurrence and other unpredictable historical accidents cause shortfalls in export receipts, leading to debt crises. Marichal does not discuss such cases. The one-sidedness of his presentation makes it difficult to recommend his book for an understanding of this topic.

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EDELBERTO TORRES-RIVAS. *Repression and Resistance: The Struggle for Democracy in Central America*. Translated by JEFF SLUYTER. (Latin American Perspectives Series, number 6.) Boulder, Colo.: Westview. 1989. Pp. vii, 165. \$28.95.

This is a collection of writings by a well-known analyst of Central American affairs. Although the introduction was written specifically for this volume, the other five essays all appeared earlier in Spanish and appear here for the first time in English translation. They deal with the root causes of the political and social upheaval that has gripped Central America in the 1980s. Most cover the region as a whole, but the fifth essay deals exclusively with the origins of the Nicaraguan revolution. Because the essays were originally published separately, there is some overlap and repetition. There are also changes in perspective, understandable considering that the earliest essay appeared in 1981 and the most recent in 1987.

Edelberto Torres-Rivas is by no means an easy writer to follow, as he himself admits, but he does have a coherent vision of what has transpired in the region. In the late nineteenth century, except possibly in Costa Rica, liberal oligarchies were established based on the holding of large estates and under constitutions that promised democracy but established authoritarianism. These oligarchies maintained their political and economic bases even through the crisis of the 1930s, but, during and after World War II, they transformed themselves into, or coopted, a modern entrepreneurial bourgeoisie. This economic transformation delayed the political crisis but created the dynamics by which democracy could have been achieved through reformist methods.

In the late 1940s and 1950s, prodemocratic forces existed that cut across class lines, but, except in Costa Rica, such democratic elements were defeated. The result was that the impulse to reform gave way to the impulse toward revolution and class struggle. Torres-Rivas nevertheless sees the rebellions in such places as El Salvador as being prodemocratic and antiauthoritarian, or at least he did in 1984 when his essay on those struggles first appeared.

The author tends to think in the abstract without giving clear historical examples of the hypothetical forces that he sees at work. He also tends to think in terms of social classes with clearly defined goals, and this way of thinking hides many complex realities. All the same, what comes through the book is that Torres-Rivas, definitely a man of the Left, has a most profound respect for democracy and democratic institutions.

THOMAS P. ANDERSON
Eastern Connecticut State University

WILLIAM B. GRIFFEN. *Utmost Good Faith: Patterns of Apache-Mexican Hostilities in Northern Chihuahua Border Warfare, 1821-1848*. Albuquerque: University of New Mexico Press. 1989. Pp. xii, 337. \$37.50.

In its conceptual scope, this monograph by William B. Griffen does not reach much beyond a journal article. Most chapters are summary descriptions of primary evidence, and nearly one-third of the book (largely consisting of two appendixes) is composed of tables of

Indian-Mexican hostile encounters. Perhaps the printing of such data will be of use to those researching the larger topic of Mexican-U.S. relations with border Indian tribes, but that alone does not justify a monographic study.

The first third of the book is largely a chronologically descriptive recording of the annual flow of hostile encounters between Mexicans in the state of Chihuahua and the various tribes composing the Apache nation (and to a lesser extent, the Comanche). The initial background chapters are superficial and often redundant. The second section of the book sets forth a "system of conflict" to be examined. Yet, although Griffen asserts that "the relationships among the several factors . . . that supported the system of conflict were complex" (p. 133), the factors are treated singularly, in separate chapters, and explained through series of descriptive details: political and military power; subsistence and trade; communication and travel; Indian and Mexican characterizations of one another; and the larger world view of each. Without proper depth, what analysis there is becomes merely suggestive or speculative. And, in a final chapter when quantification is employed to probe the topic, the data are only summarily described. No attempt is made to uncover the interrelationships within the data or their connection to the variables enumerated as composing the "system of conflict."

The underlying narrowness of conceptualization appears to stem from the limited primary evidence. Griffen relies on three archival sources: Chihuahua's official newspapers and documents from two presidios. The primary sources that he lists but did not consult are evidence enough of a bibliographical failing: national defense archives in Mexico City and state and local archives in Chihuahua. Moreover, no use is made of U.S. consular reports, and very few travelers' accounts are employed. The same inadequacy is found in his use of secondary works. Other than the author's own publications, only fifty-four separate items are cited, twenty-nine of which were published before 1970. Is there not a point beyond which one must go, in considering primary sources and secondary works, before an adequate analysis for a monographic study can be rendered?

Although the author acknowledges the essentially descriptive character of the book (p. 218), his assertion of its narrative quality can only be justified in a limited sense of the word. This account of Indian-Mexican border relations does not constitute a good story told. Without a tight plot or development of characters, or a literary style to weave them together, this book is largely an annotated, paraphrased collection of data.

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STEPHEN H. HABER. *Industry and Underdevelopment: Industrialization of Mexico, 1890-1940*. Stanford: Stanford University Press. 1989. Pp. xiv, 237. \$35.00.

In this study, Stephen H. Haber looks at fifty years of Mexican industrial development. As the basic unit of analysis, he has adopted the industrial firm itself, shifting the perspective from which he approaches this problem away from government policy, national economic trends, or workers' rights to that of the companies themselves, though he keeps these other factors in mind. In proceeding in this way, he develops some important new interpretations about the Mexican economic scene, particularly for the period of the Mexican revolution, 1910-20, and for the presidency of Lázaro Cárdenas, 1934-40.

The larger portion of this book is devoted to an analysis of industry in the Porfirian period, 1890-1910. Fully seven chapters of the total eleven furnish a detailed look at the major businesses that dominated the Mexican economy during that period of time. In these valuable chapters, Haber shows that a small group of merchant-financiers, mostly foreign, invested in large-scale enterprises that were unable to compete effectively with external competition. They therefore adopted a strategy in which they manipulated the political and economic system to create a climate in which these enterprises would become profitable. Tariff protection and tax exemptions were forthcoming from the Díaz government, which was heavily influenced by this small clique of merchant-financiers. Indeed, Haber maintains that "it was not so much a question of the state representing the interests of these financiers as it was that these financiers *were* the state" (p. 69). In addition, "they exploited their financial and political advantages to discourage competition" (p. 84). In this effort they were notably successful, establishing national or regional monopolies in both consumer and intermediate goods.

It is in the later chapters, however, that Haber's new insights are most striking. First of all, he challenges the idea that many physical plants were destroyed during the violent years of the revolution. On the contrary, he says, most large manufacturing companies survived extremely well. Mexico's revolutionary leaders were smart enough to know that the survival of factories and productive facilities in their areas were important to their own efforts, and most of these industries were retained by their original owners. What changed was the attitude of these owners. No longer believing that the economic future was bright and that years of progress lay ahead, they slowed down new investment considerably, in a process that in some cases actually amounted to disinvestment. Through 1925, new companies made up some of the difference, but a period of depression between 1925 and 1932 stopped this new investment as well. The Mexican economy did not begin to recover until 1933 in a process that accelerated during the presidency of Cárdenas.

Haber shows convincingly that Cárdenas, who has

long had a reputation of being antibusiness, actually pursued policies that led to recovery despite the worldwide depression of the 1930s. Helped by the rise in export prices, particularly of oil, gold, and silver, Cárdenas put into place an expansionary monetary policy that permitted a fall in interest rates and a rise in consumption. The peso was allowed to float, and, as it lost value against the dollar, an implicit tax on imports was created that favored Mexican products. Moreover, from 1935 on, the Mexican government deliberately ran deficits as part of its efforts to halt the economic depression. A program of spending on infrastructure not only enhanced the ability of the nation to produce and transport goods but also gave a boost to producer goods industries such as steel and cement that provided materials for construction. Confidence improved and investment grew. Cárdenas's policies, despite the usual historical perception that he was anticapitalist, improved rather than jeopardized the business climate. Furthermore, his labor policies brought organized labor under the control of the state, furnishing a measure of social peace that was good for business. Even his expropriation of the foreign oil companies does not seem to have been discouraging to Mexican manufacturers.

Haber has provided us with an important new look at the history of Mexican industry. His careful discussion of the nature of the industrial system emerging in the latter part of the nineteenth century and his insights into the revolutionary and postrevolutionary periods provide an excellent basis for a better understanding of the rapid expansion of Mexican industry after 1940.

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LOUIS A. PEREZ, JR. *Cuba: Between Reform and Revolution.* (Latin American Histories.) New York: Oxford University Press. 1988. Pp. xii, 504. \$24.95.

Among historians of Latin America, Cuban specialists bear a scholarly burden once shouldered mostly by Mexicanists, that is, explaining revolution, its origin, meaning, and legacy. By and large, the latter have pretty much succeeded in placing the Mexican revolution within the context of Mexican history. Lamentably, Cuban studies have not yet achieved this level of scholarly development. The reasons are numerous, but one more than any other stands out, namely, the Cuban revolution and its impact on modern international politics. Within the academic community, Fidel Castro's triumph was electrifying and generated immediate benefits. Instructors invigorated once-dreary lectures with fresh material, Latin American classes began to fill up, and Latin American specialists found a new calling and an easier route to getting into print. But there was an unfortunate (and, in some cases, tragic) outcome to this process. A few, bitten by the Cuban bug, never recovered; for them, 1959 is not only the turning point in Cuban history but also the turning

point in history. Others became so embittered over the "betrayal" of the revolution that they have committed their professional lives to what they contend is the pursuit of truth but what is in reality a vengeful quest.

Louis Pérez, Jr., falls into neither of these camps, yet he is not neutral about Cuba or its history. In this study he has persuasively and authoritatively shown that Castro's revolution must be placed in the context of Cuba's revolutionary history. To understand what the Cuban revolution identified with Castro means, Pérez argues, requires a recounting of the revolutionary struggles in Cuban history from the early years of the colonial period. Until the eighteenth century these were largely obscure acts of defiance of the colonized against their masters within the Spanish empire, but, with the *veguero* rebellion against Bourbon authority in the eighteenth century, they became something more ominous for those powers that have striven to subdue Cuba through either reform or repression.

The efforts of the larger powers—Spain, Great Britain, the United States, and even the Soviet Union—to "tame" Cuba with economic or military means have occupied center stage in the recounting of Cuban history and have obscured the struggle within Cuba against foreign domination and the more significant debate among Cubans themselves about *Cubanidad* and national destiny. For example, eastern Cuba developed a revolutionary tradition early on, from the age when its isolated and neglected peoples traded clandestinely with non-Spanish contrabandists until Castro entered the region in the 1950s and discovered a generation of abused peasants who had already raised the flag of defiance against central authority in Havana. Spanish Cuba was a colony within a colony. The network of privileged Spanish bureaucrats and merchants roused the Creoles to voice a muted protest, but it did not bring a successful struggle for independence, as elsewhere in Spanish America.

The most obvious reason for Cuba's long delay in achieving independence, most scholars have argued, lay in its economic and political vulnerability in the international arena. Pérez acknowledges the importance of external factors but looks within Cuba to find the central determining forces in the long and often-tortured struggle for true independence. The key to unlocking the Cuban past, he writes, is understanding the dynamic of the revolutionary tradition on the island at every critical epoch—in the 1840s and 1850s, an era of slave rebellion and powerful arguments for annexation by the United States; in the Ten Years' War, when separatist Creoles tried to mollify fearful slaveholders by pledging to respect private property; in the 1890s, when revolutionaries seized the initiative from autonomists only to be denied victory and true independence by the intruding American military; in the first decades of the stunted republic, when the U.S. economic and political presence lay so heavily on them that Cubans found gainful employment only in public service; in the Batista era, which commenced in revolutionary promise in 1933 and ended in revolutionary

triumph in 1959. In each of these periods, the revolutionary impulse succumbed to the reformist solution, which, as Pérez makes clear, was generally a means of preserving the social structure and the foreign connection. In the process, *Cubanidad* suffered, but the revolutionary tradition survived.

Some may fault Pérez for assigning only one chapter of twelve to socialist Cuba, as if to slight intentionally its impact or perhaps to accommodate the publisher by keeping the book within manageable limits. I believe that neither is the case, however. Rather, Pérez has placed Castro's revolution within the Cuban revolutionary tradition, which reaches deep into Cuban history. And he has earned for himself the designation of premier Cuban scholar in the United States.

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University of Georgia

RODERICK J. BARMAN. *Brazil: The Forging of a Nation, 1798-1852*. Stanford: Stanford University Press. 1988. Pp. vi, 334. \$39.50.

Unlike the colonial elites of the British and Spanish New World empires, those of the Portuguese empire managed to preserve its territorial integrity, forming Brazil in 1822 as a single sovereign state. This book is an examination of the actions of that elite that led to this singular outcome; Brazil was finally consolidated by the mid-nineteenth century. Roderick J. Barman demonstrates convincingly that this outcome was not inevitable: the captaincies into which the colony had been divided were as much the object of nativist patriotism as those of the Spanish colonies, their interests were strikingly divergent, they were geographically remote from one another, and some of them had special reasons for maintaining links with the metropolis. This nation, furthermore, imposed new inequalities in place of the old: the capital of Rio de Janeiro was as economically parasitical as Lisbon had been, and the elite entirely rejected Western egalitarianism and insisted on preserving the slave trade. Brazil was therefore confronted with repeated rebellions by local landowners, petty traders and artisans, and the great underclass of slaves, backwoodsmen, and army mercenaries.

This is ground that has been much traversed by Brazilian historians. A literature of the independence period, securely based on abundant and well-preserved documentation, was generated beginning in the last century to shore up the legitimacy of this extraordinarily exclusionary nation-state. More recently this traditional school has been revised by a generation of political historians, led by José Honório Rodrigues, seeking to recapture the past in behalf of the popular majority. The author's selection of materials from the vast stock of primary sources and his judicious reanalysis of these conflicting interpretations are impressive.

The pressures exerted on the Portuguese regime by the French revolution and Napoleonic expansionism,

leading to the flight of the court to Rio de Janeiro, provoked a chain of events culminating in a precarious independence under an emperor who was the son of the Portuguese king. A rush of unpremeditated and contradictory decisions left every important political question unsettled. A generation was required to define national boundaries (Uruguay, a conquered province, was lost), break free of the tutelage of the British, sever dynastic ties with Portugal, define a constitutional role for the monarch, organize a party system, adjust the legal structure to the demands of the provincial notables, reconstruct a bureaucracy capable of imposing its will throughout the empire, and suffuse the result with an air of inevitability. None of these successes in retrospect please the eye of the democrat: Barman reveals an entrenched political oligarchy, slavery triumphant, popular education nonexistent, elections pure fraud and violence, and nepotism and graft the meat and drink of politics.

All of this is set forth shrewdly and insightfully. Perhaps more attention might have been given to the military, which, unlike the armies of Spanish America, was subordinated to the civilian central government; to the abdication of control over public lands, which permitted the replication of the latifundia; and to the failure to attract European immigrants, which so remarkably contrasts with the contemporary social formation of the United States. This book is, nevertheless, an excellent monograph, well conceived and executed, of great value to the comparative study of the development of the nation-state in Africa and Asia as well as Latin America.

WARREN DEAN
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YOUSSEF COHEN. *The Manipulation of Consent: The State and Working-Class Consciousness in Brazil*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1989. Pp. ix, 185. Cloth \$29.95, paper \$12.95.

Youssef Cohen argues that Brazilian workers prefer authoritarian military dictatorship to civilian democracy and that they hold this preference because dictatorship was "insidiously" imposed on them in the 1930s by the "state elites" of the Estado Novo (p. 4). Support for the first proposition comes from a survey taken in 1972 and 1973 of 617 urban workers in southeastern Brazil. Despite the fact that this is a book that purports to tell us about working-class consciousness, not a single worker is mentioned, named, or quoted, and it is unclear whether any were given the opportunity to express an opinion in terms other than those provided by the survey. One wonders whose consciousness is actually on display.

Cohen reports the aggregate responses to seven questions, one of which, curiously, is stated differently at separate points in the book (see pp. 40, 125). These data provided the basis for a useful journal article ("The Benevolent Leviathan: Political Consciousness

among Urban Workers under State Corporatism," *American Political Science Review* [1982]: 102–14) but are insufficient to sustain a monograph. A subsequent chapter claims to show how the corporatist labor arrangements of the Estado Novo "indoctrinated" Brazilian workers in the authoritarian "state ideology." No evidence is presented concerning such indoctrination, however; instead, Cohen simply summarizes the institutional evolution of Brazilian labor relations from the 1930s through 1964, drawing heavily on the work of Kenneth Erickson.

A penultimate chapter, based largely on John Humphrey's book on São Paulo autoworkers, examines the rise of the "new unionism" of the 1970s and 1980s and concludes that, "should Brazil remain democratic, the future does not hold much promise for the state ideology" (p. 115). This statement comes as a bit of a surprise after one hundred pages in which we are repeatedly informed that, for the last forty years, Brazilian workers have been in thrall to an authoritarian ideology imposed on them from above. Now, it seems, they will put that ideology aside and move on to something better.

If Brazilian workers are in fact subject to an authoritarian ideology, that ideology did not originate in the 1930s. This book makes no mention of the rich historical and anthropological literature documenting authoritarian aspects of Brazilian social relations, which have deep roots stretching back to the experiences of slavery and colonial rule. And, in positing an "underprivileged" class that passively, and even gratefully, accepts whatever ideological constructs are imposed on it from above, Cohen ignores an equally rich body of literature that has examined the complex and almost infinitely variable dialectic between elite domination (both inside and outside the state) and popular responses to such domination. Historians acquainted with either of those literatures will find reading this book a frustrating and ultimately unrewarding experience.

GEORGE REID ANDREWS
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PETER BAKEWELL. *Silver and Entrepreneurship in Seventeenth-Century Potosí: The Life and Times of Antonio López de Quiroga*. Albuquerque: University of New Mexico Press. 1988. Pp. xviii, 250. Cloth \$35.00, paper \$15.95.

This richly textured life-and-times biography of Antonio López de Quiroga, the seventeenth-century Potosí entrepreneur, brings a broad new dimension to our understanding of mining in colonial Hispanic America and many new insights into the society and economy of one of the most important cities in the world during the *ancien régime*. López made an indelible mark on Potosí, the bustling upper Peruvian city where this Spanish Galician put down his roots during the 1650s. As large as many major cities of Europe—Venice, Marseilles, Seville, or Amsterdam—Potosí was dominated by silver

production, and López quickly became involved in the mining sector, first as an *aviador*, who provided credit to silver miners, and then as a silver broker or trader (*mercador de plata*), who bought bulk silver from miners and refiners for minting into coins. In 1657 López became a mine owner himself, and, by the opening of the next decade, he had extended his mining activities enough to qualify as an *azoguero*, a buyer of mercury for the amalgamation of silver. In fact, in the 1660s he purchased over 13 percent of the quicksilver sold in Potosí. Expansion of López's mining operations stemmed from his ability both to open new veins of silver ore and to reopen older ones. Innovative use of blasting techniques to ventilate, drain, and create accessible new shafts was at the heart of his success. His penchant for reinvesting his mining profits immediately, caution and prudence in not overextending himself, ability to choose good managers from among family and friends, and a ready supply of capital also served him well as he steadily expanded his holdings—and his profits. Overall, from 1660 until his death in 1699, Antonio López de Quiroga was responsible for an astounding one-seventh to one-eighth of the silver output in Potosí, almost one hundred million U.S. dollars in 1987.

López's success in mining also led him into other ventures. With his mining profits, he bought land that produced livestock, wine, white and black fat, coca, maize, and dried potatoes for the Potosí market. His son-in-law Miguel de Gambarte involved López in the long-distance trade with Buenos Aires and perhaps the smuggling of contraband silver east to the Atlantic coast. López's increasing wealth, however, was not enough to insure his successful quest either for a title—count, marquis, and *adelantado* of Pilaya y Pazpaya—or for a post as alderman on the Potosí city council. Although he became a captain and then a colonel (*maestre de campo*), his gifts lay more in mining and business than in the military, as he failed miserably to conquer the Gran Paititi in eastern upper Peru.

This graciously written book is far more than a simple biography. It provides significant new insights not only into the entrepreneurial aspects of the Potosí mining scene but also into the ways in which silver fueled the local economy. With its rich, multidimensional picture of life in Potosí, this book is also social and urban history at its best. On a broader scale, Peter Bakewell demonstrates that the late seventeenth century was not a long noonday nap for the Spanish empire as generally supposed, even in Potosí where mining production was falling. Moreover, he challenges the stereotypical view that Spanish colonials lacked entrepreneurial abilities and attitudes. In sum, this is a keen analysis of the mining economy and society of seventeenth-century upper Peru in particular and Hispanic America in general.

JOHN JAY TEPASKE
Duke University

ROSA DEL CARMEN BRUNO-JOFRE. *Methodist Education in Peru: Social Gospel, Politics, and American Ideological and Economic Penetration, 1888–1930*. (Comparative Ethics Series, number 2.) Waterloo, Ontario: Wilfrid Laurier University Press, for the Canadian Corporation for Studies in Religion. 1988. Pp. xiii, 223. \$18.50.

Did Methodist education have any influence on Peruvian thought? According to Rosa del Carmen Bruno-Jofré, the answer is yes. She uses as evidence the official correspondence of the American Mission of the Methodist Episcopal Church in Peru. A historian by training, she wrote this work in Canada in 1982 and 1983.

Methodist influence in Peru before 1930 was asserted primarily through Methodist schools. Methodist schools taught English and commercial subjects, insisted on coeducation, awarded scholarships to children of poor families, promoted extracurricular activities, and took students on field trips. Peruvian middle-class parents, although Roman Catholic, sought that kind of education for their children. Methodist educators encouraged parents to participate in school activities and formed alumni associations. They applied the tenets of the Social Gospel, progressive reforms, and John Dewey's democratic and pedagogic ideas. In addition, Protestant missionaries educated Quechua- and Amara-speaking Indians. Bruno-Jofré ends her book with a good discussion of Methodist schools, programs, finances, teachers, and curricula. She concludes that Methodist education promoted "imperialist capitalism" and created a middle-class work force that ended up working for an "oligarchic state" that it opposed (p. 175).

Bruno-Jofré's most original insight is her discussion linking the leaders of the American Popular Revolutionary Alliance (APRA) to Protestants. Raúl Haya de la Torre and other *Apristas* taught at the Anglo-Peruvian school and at other Protestant schools, and they supported the Young Men's Christian Association movement. Bruno-Jofré persuasively argues that the *Apristas* espoused Protestant ideas of discipline, obligation, the Social Gospel, and democracy.

The influence of the Methodists and other Protestants is all the more remarkable because there were few converts to Protestantism before 1930. Bruno-Jofré gives reasons for the few conversions to Protestantism. She does not distinguish Protestantism from evangelicalism. Curiously, neither she nor other scholars of pre-1930 Latin America have an inkling of today's

phenomenal growth (at a rate of four hundred converts per hour) of evangelical and fundamentalist Protestantism in Latin America.

Bruno-Jofré is of two minds about Methodist and Protestant influence in Peru, which weakens the coherence of her argument. Although she admires Protestant schools, hospitals, and democratic ideals, she also deplores them as aiding North American imperialism and capitalism. Marxist jargon interferes with the narrative: words such as "petit bourgeoisie," "pre-capitalist," "feudal," and "imperialist" are sprinkled throughout without being defined. Ironically, an American company, the Cerro de Pasco Copper Corporation, asked the Methodist mission to leave in 1941 because the Methodists were associating with the populist APRA. That request seemingly contradicts the thesis that the Methodists paved the way for capitalist imperialism.

Bruno-Jofré never explains why 1930 appears in the title as a cutoff date, except that it is the last year of Augusto Leguía's eleven-year presidency. Nor does she discuss whether and how Methodist education changed after 1930. That omission is especially puzzling because she includes data from the 1940s and 1950s.

Bruno-Jofré provides a synopsis of all the major religious events affecting Peru from 1888 to 1930. For instance, she includes the 1916 Panama Congress on Christian Work in Latin America, which defined the aims and ideals of Protestant education. Fewer than 5 percent of the Protestant delegates were Latin Americans. A subsequent history of Methodist education in Peru would have to note a successor congress, the Second Assembly of the Latin American Council of Churches, which met in Brazil in 1988 and at which 90 percent of the representatives were Latin Americans. It is estimated that one-eighth of the 418 million people in Latin America now belong to Protestant groups, which especially attract the poor. Clearly, a sequel to this book could no longer view Protestantism as "bourgeois" and "North American."

Although Bruno-Jofré is ambivalent about the role of Protestantism in Peru, she makes a welcome contribution to the history of Protestant education in Latin America. She points to new avenues for further research through content analysis of denominational archives.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

RICHARD RODGER, editor. *Urban History Yearbook, 1989*. New York: Leicester University Press of Pinter; distributed by Columbia University Press, New York. 1989. Pp. x, 294. \$39.00.

D. I. GREENSTEIN, Politics and the Urban Process: The Case of Philadelphia, 1800–54. M. D. REILLY, Urban Electric Railway Management and Operation in Britain and America, 1900–14. SALLY-BETH MACLEAN, Drama and Ceremony in Early Modern England: The REED Project. JAMES ALEXANDER, The Economic Structure of the City of London at the End of the Seventeenth Century. DENNIS MILLS and JOAN MILLS, Occupation and Social Stratification Revisited: The Census Enumerators' Books in Victorian Britain. HERMAN DIEDERIKS, Economic Decline and the Urban Elite in Eighteenth-Century Dutch Towns: A Review Essay. CARL STRIKWERDA, The Paradoxes of Urbanization: Belgian Socialism and Society in the *belle époque*. RICHARD RODGER, Conference Reports. PETER BORSAY and BILL LUCKIN, assisted by MALCOLM HOGG, Review of Periodical Articles. DAVID REEDER, Research in Urban History: A Review of Recent Theses.

SUSAN ZIMMERMAN and RONALD F. E. WEISSMAN, editors. *Urban Life in the Renaissance*. (Center for Renaissance and Baroque Studies, University of Maryland, College Park.) Newark, N.J.: University of Delaware Press. 1989. Pp. 303. \$47.50.

SUSAN ZIMMERMAN and RONALD F. E. WEISSMAN, Introduction. JAMES S. ACKERMAN and MYRA NAN ROSENFELD, Social Stratification in Renaissance Urban Planning. NICHOLAS ADAMS, The Construction of Pienza (1459–1464) and the Consequences of *Renovatio*. BARBARA DIEFENDORF, Houses Divided: Religious Schism in Sixteenth-Century Parisian Families. ROGER CHARTIER, Leisure and Sociability: Reading Aloud in Early Modern Europe. WERNER L. GUNDERSHEIMER, Trickery, Gender, and Power: The *Discorsi* of Annibale Romei. HOWARD MAYER BROWN, Minstrels and Their Repertory in Fifteenth-Century France: Music in an Urban Environment. ARTHUR K. WHEELLOCK, JR., History, Politics, and the Portrait of a City: Vermeer's *View of Delft*. DAVID HARRIS SACKS, Celebrating Authority in Bristol, 1475–1640. SYDEL SILVERMAN, The Palio of Siena: Game, Ritual, or Politics? RICHARD C. MCCOY, "Thou

Idol Ceremony": Elizabeth I, *The Henriad*, and the Rites of the English Monarchy. RONALD F. E. WEISSMAN, The Importance of Being Ambiguous: Social Relations, Individualism, and Identity in Renaissance Florence. THOMAS M. GREENE, Ceremonial Play and Parody in the Renaissance.

JOHN WALTER and ROGER SCHOFIELD, editors. *Famine, Disease, and the Social Order in Early Modern Society*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 10). New York: Cambridge University Press. 1989. Pp. xiv. 335. \$49.50.

JOHN WALTER and ROGER SCHOFIELD, Famine, Disease, and Crisis Mortality in Early Modern Society. JOHN WALTER, The Social Economy of Dearth in Early Modern England. KEITH WRIGHTSON and DAVID LEVINE, Death in Whickham. PAUL SLACK, The Response to Plague in Early Modern England: Public Policies and Their Consequences. JACQUES DUPAQUIER, Demographic Crises and Subsistence Crises in France, 1650–1725. DAVID R. WEIR, Markets and Mortality in France, 1600–1789. E. A. WRIGLEY, Some Reflections on Corn Yields and Prices in Pre-industrial Economies. ROGER SCHOFIELD, Family Structure, Demographic Behaviour, and Economic Growth.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

REVIEWS OF BOOKS

TO THE EDITOR:

In the *American Historical Review*, 95 (April 1990): 542, William W. Hagen, University of California, Davis, reviewed my book *Rozwój organizacji społeczno-narodowych wychodźstwa polskiego w Niemczech w latach 1870–1914* [The Development of Social and National Organizations among Polish Immigrants in Germany, 1870–1914] (1987).

This critical review extends beyond the scope and the topic of my book, because Hagen expresses his negative opinion about Polish historiography generally. He does not recognize that during the Communist period it was still possible to write some valuable works in Poland. Many are at a high level of research and are without the Marxist-Leninist method of analysis and dogma.

He suggests that my book evokes an uncritical spirit of nationalism in the past. Indeed, there was strong anti-German and anti-Russian sentiment in 1870–1914 among the Polish people. But where I emphasize this in my book, I mean it as an example of national consciousness during the phase of Polish national development that must be observed and researched by historians of this period regardless of what their opinions on this phase may be.

Moreover, Hagen omitted the word "national" from the title of my book, thereby giving a misleading idea of its subject. [The *AHR* staff translates the book titles.—The Editor] The development of Polish national consciousness was slow at the beginning, around 1870–1890, and faster in the early years of the twentieth century until 1914.

Pan-Polish (*narodowopolski*) does not mean "Great-Polish" and is not, in any case, a kind of "intolerant

chauvinism," as Hagen implied. It was understood as patriotic behavior that was anti-German only in self-defense against German politics, which at that time was strongly anti-Polish and which had as its target the extinction of the Polish minority in Germany. There is no evidence of anti-Semitism in this period such as Hagen suggested in his review.

Since my book ends at 1914, his criticisms that it is nationalistic and that it ignored the "intolerant chauvinism" of the National Democratic movement testify, along with his mistranslations and quotations out of context, to a complete misunderstanding of the topic and scope of my work.

JERZY KOZŁOWSKI
Poznań

WILLIAM HAGEN REPLIES:

I am sorry that Jerzy Kozłowski, whose "good intentions and industriousness" I saluted at the outset of my review, does not see fit to recognize those qualities in me. I have, as he should know, read a great deal of Polish Marxist historiography and know how to value the best of it. Indeed, I referred favorably in the review to Krystyna Murzynowska's book of 1972 on the Poles in Imperial Germany's Ruhr district. This is not to say that Marxist-Leninist dogmatism did not disfigure many books published in People's Poland. Certainly it did. My point was precisely to draw attention to the significance of Kozłowski's dismissal, in a book published in 1987 under the waning Communist regime, of Marxist-Leninist analysis in favor of an uncritically nationalist historiography, a substitution that, in my view, is very far from representing any kind of improvement.

Kozłowski obligingly illustrates my thesis by maintaining, in his letter, that Pan-Polonism before World War I did not entail "intolerant chauvinism." Perhaps Kozłowski has never heard of Roman Dmowski and the Pan-Polish National Democrats, whose following was second to none among political movements in Prussian Poland. Dmowski and his friends raised intolerant chauvinism to a high virtue precisely in the pre-1914 period. Allied with the National Democrats were numerous Polish populists (*ludowcy*) animated by strident

xenophobia. To excuse this, as Kozłowski does, as Polish "self-defense" is to screen out the question—vital to the political and cultural future of post-Communist Poland—of the harm that the Poles have inflicted upon themselves, and others, through their nationalist enmities and obsessions. This point Kozłowski underscores by his refusal to acknowledge the existence of Polish anti-Semitism in the pre-1914 period. In fact, anti-Semitism—both scurrilous and polite—was widespread, as anyone familiar with the politics and journalism of the Prussian Poles knows (and as most Polish Marxist histories readily, and even regretfully, concede). To take a friendly view of the Jews was to risk denunciation by "Pan-Polish" circles for a lack of the "patriotic behavior" Kozłowski so warmly recommends. (Readers interested in documentation of Polish hyper-nationalism and anti-Semitism, alongside other, more reasonable political positions, will find it in my *Germans, Poles, and Jews: The Nationality Conflict in the Prussian East, 1772–1914* [1980]).

WILLIAM W. HAGEN
University of California, Davis

TO THE EDITOR:

Robert James Maddox's review of William E. Pemberton's *Harry S. Truman: Fair Dealer and Cold Warrior* (*AHR*, 95 [June 1990]: 939) is a disgrace. I am surprised that you let your standards fade in this case. Laced with flippant comments and sarcasms that we try to teach our students to avoid in serious writing, the review is little more than a diatribe against an interpretation Maddox disputes.

Maddox barely contends with Pemberton's provocative themes. Maddox's use of selective quotations to bash the book is reprehensible. For example, he takes Pemberton to task for writing, according to Maddox, that "[t]he atomic bomb dropped on Hiroshima killed 'thousands of women and children and a few prisoners of war.'" Actually, Pemberton wrote on page 55 of his book, "Recent estimates are that it [the atomic bomb on Hiroshima] killed 130,000, including thousands of women and children and a few American prisoners of

war" (emphasis added). Maddox deleted, without telling the reader he had done so, two key words. Moreover, Pemberton mentions women and children, because, on the previous page, he explained that the bomb was supposed to be used against a military target, not innocent civilians.

There is much to argue with in Pemberton's brief biography—in fact, I assign it in an undergraduate class because, as Richard Kirkendall says in a dust-jacket blurb, Pemberton's book "challenges" a number of "Truman myths." It is a serious book, Maddox should have taken it seriously, and the *AHR* should have published a serious review.

THOMAS G. PATERSON
University of Connecticut

ROBERT MADDOX REPLIES:

Thomas Paterson's complaint about "flippant comments and sarcasms" is awkward in defense of a volume with chapter titles such as "The Truman Merry-Go-Round" and "The Russians Are Coming." The tone of the review was entirely appropriate for the book, which is flippant and sarcastic throughout. Readers should know that Paterson's enthusiasm for *Harry S. Truman* may be a tad influenced by the fact that Pemberton flogs the same themes about the cold war that Paterson has been flogging for twenty years.

Paterson's recently acquired concern for accurate quoting is welcome. Seems like only yesterday he was dismissing such matters as nitpicking when they were raised with regard to revisionist writing. He blows smoke here, however. The fact remains that when Truman orders the attack on Hiroshima, Pemberton reminds his readers of the women and children killed. When the Berlin airlift is mounted, the only named beneficiaries are the "many recently returned from slaughtering Americans in World War II or from running the camps at Dachau and Bergen-Belsen." This sort of thing permeates the book.

ROBERT JAMES MADDOX
Pennsylvania State University,
University Park

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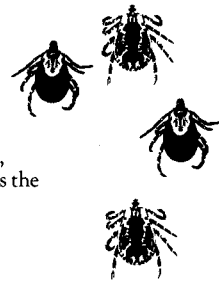
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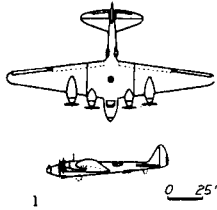
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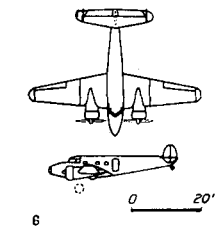
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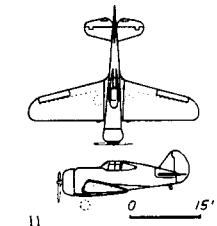
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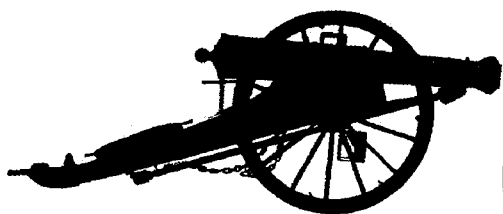
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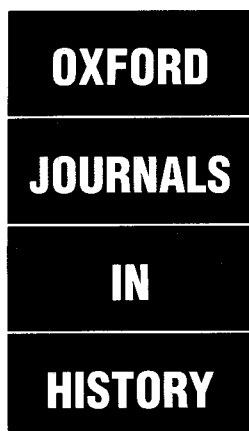
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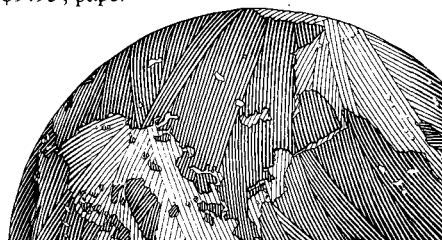
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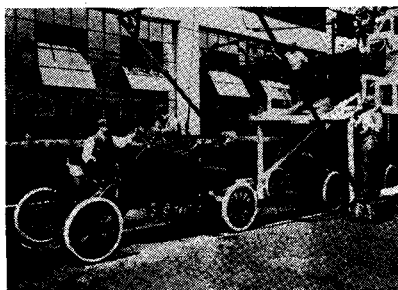
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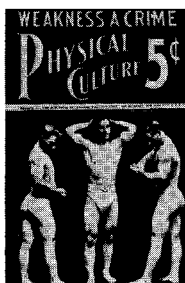
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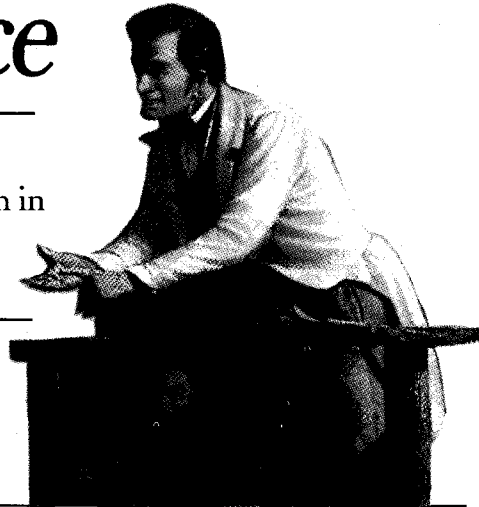


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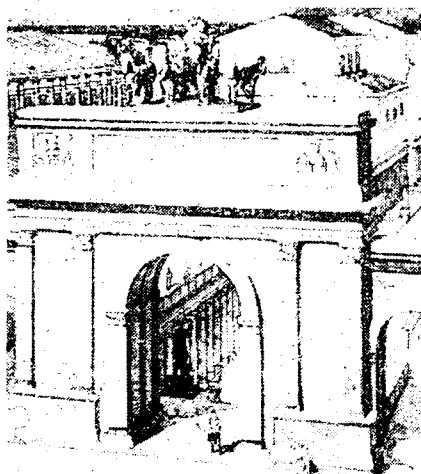
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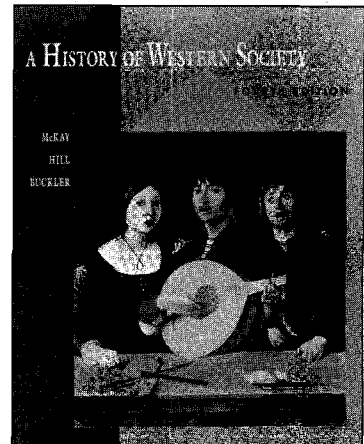
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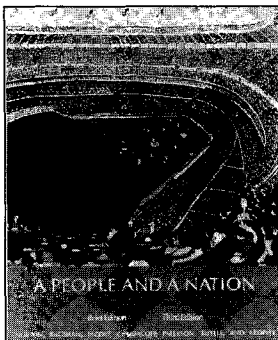
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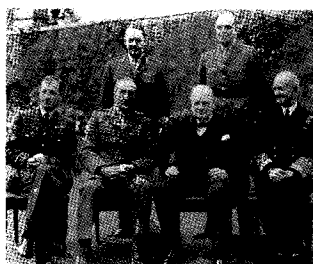
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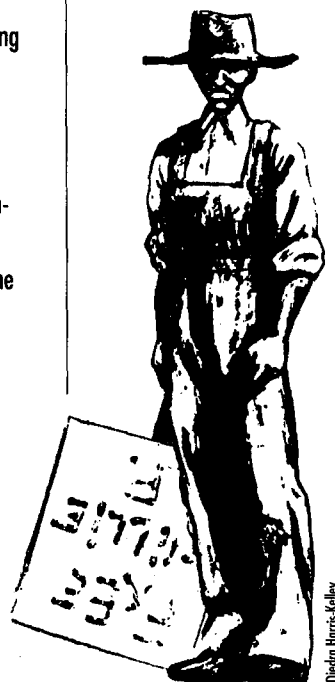
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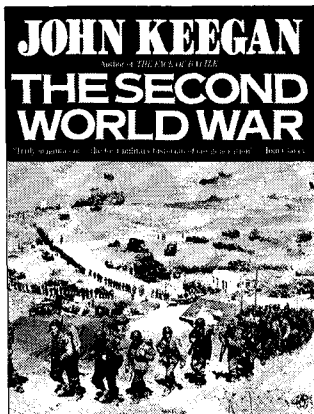
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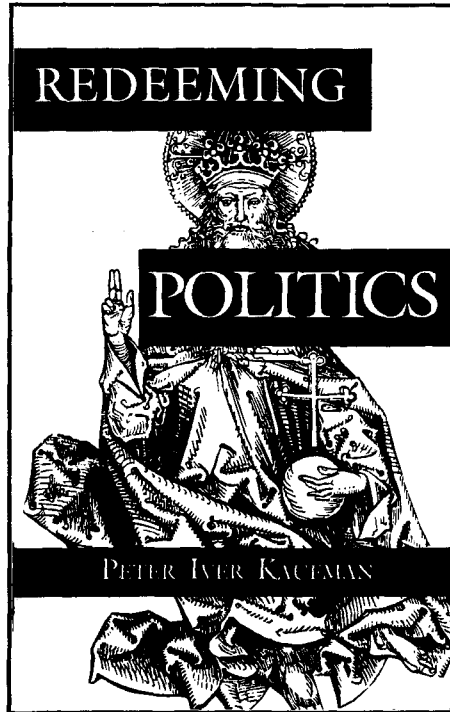
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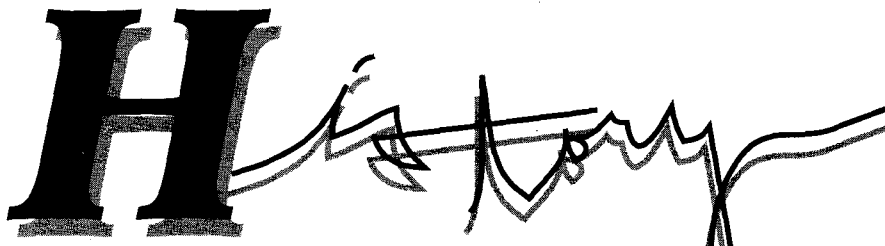
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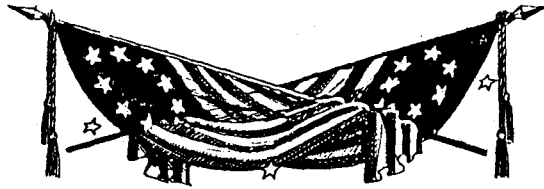
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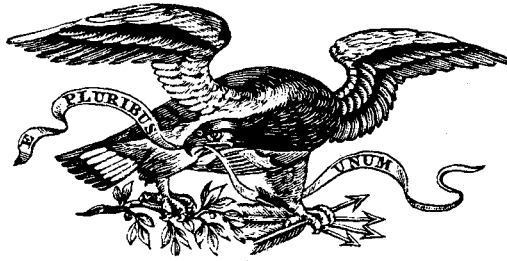
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